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ETHNIC AND PARTY AFFILIATIONS OF CANDIDATES AS DETERMINANTS OF VOTING[1]

LEON J. KAMIN

McMaster University

THE PROCESS of electoral choice in a representative democracy ultimately confronts the voter with a ballot form presenting a number of alternative candidates among whom he must choose. The bases upon which such choices are made are an obvious concern of political theory and also a legitimate subject of psychological inquiry. Political theory tends to stress party affiliation as a rational determinant of such choices; political parties are said to represent distinctive programmes of action, and the voter, by choosing the candidate of the appropriate party, expresses his preference among these programmes. There are, however, many other factors which undoubtedly influence the voter's choice (e.g., 1, 2, 3).

Rationally irrelevant factors such as a candidate's personal appearance, mannerisms, and sex may influence voters in one direction or another. Another factor is perhaps of greater moment; practical politicians assert with confidence the importance of the ethnic affiliations of candidate and of voter. Voters of French descent are said to prefer candidates of French origin, voters of Jewish descent, Jewish candidates, and so on. The basis for such assertions lies largely in the analysis of actual election returns: French-speaking districts tend to return French-speaking candidates, Jewish districts Jewish candidates.

The study of such factors by analysis of actual elections suffers from inherent difficulties. Thus, if an Anglo-Saxon district traditionally returns an Anglo-Saxon Liberal, it is by no means clear whether this is because the candidate is Anglo-Saxon, is Liberal, wears a goatee, or all of these. We cannot, of course, intervene in actual elections to separate such confounded factors, but we can arrange artificial elections in such a way as to separate those factors whose effects we wish to study.

The essential technique of the present study was to approach voters in the context of a public opinion poll before an actual parliamentary election, and ask them to make a choice among fictitious candidates for a fictitious office. The personal characteristics of the "candidates" could obviously be of no influence in such a choice; hence, by varying in

[1]This research was supported by a grant from the Arts Research Committee of Queen's University, where the work was performed. Thanks are due to Messrs. J. W. S. Becker and Charles Hockman for assistance.

permutation their alleged ethnic and party affiliations, clear tests of the relative importance of these factors could be made. Although the conclusions drawn from the study are limited to the specific samples employed, the methodology may be of more general interest.

Method

The respondents' voting choices were obtained by door-to-door canvassing during May and June, 1957, before the June parliamentary election. The canvasser, carrying a cardboard "ballot box" and a sheaf of mimeographed ballot forms, asked the householder if he were a registered voter. If so, the canvasser explained that an attempt was being made to predict the outcome of the forthcoming election. The householder was asked to participate in the poll by accepting a ballot form, marking his choices, and depositing the ballot anonymously in the box. The refusal rate was very low and, in any event, has no bearing on the following analyses.

There were two separate polls, one conducted in the city of Kingston and one in the city of Cornwall. The Kingston canvasser was a native English speaker, and was instructed to avoid households with nameplates obviously suggesting French descent. The composition of the Kingston population makes it certain that very few of the respondents were ethnically French, and suggests that the majority were Scottish, English, and Irish. The Cornwall canvasser was a native French speaker, instructed to remain within a French-speaking neighbourhood. The Kingston ballots were in English, the Cornwall ballots in French. We shall refer to the respondents in the two cities as, respectively, "English" and "French."

The general form of the ballots employed is illustrated in Figure 1, which reproduces *one* of the forms used in Kingston. The ballot presents two separate contests. The first is genuine, giving the names and party affiliations of the actual candidates for parliament. The names and parties of the true candidates were never tampered with, though on some ballot forms there was no mention of political parties. The second contest, for "Federal Solicitor," is wholly fictitious. The parties attributed to the three fictitious candidates were permuted on different ballot forms. There were some ballot forms which made no mention of parties; on forms of this type the orders in which the three fictitious names appeared were permuted.

The canvasser was thus equipped with a number of different ballot forms. Within any given neighbourhood, he used the various forms in sequence as he progressed from house to house. The data analyses will be based on comparisons between different ballot forms, and for this purpose the technique assures equivalent samples for different forms. Independently of this, an attempt was made to sample geographic neighbourhoods in such a way as to be roughly representative of the entire city. The distribution of respondents' choices in the genuine contest, it may be noted, corresponded very closely to the votes later cast in the real election in the city.

The Cornwall ballots were of the same general construction as the Kingston ballots, though the names of the true candi[illegible]es for parliament were, of course, different. The fictitious office was termed "Co[illegible]leur Juridique Fédéral." The three fictitious candidates, whose political parties a[illegible] positions on the ballot were permuted, were Lucien Beaulieu, Robert H. Har[illegible], and René Lavoisier. The canvasser made no attempt to obtain a representative sample of the city of Cornwall, remaining instead within a single French-speaking neighbourhood. In this neighbourhood different ballot forms were used in sequence to obtain equivalent samples.

THE PUBLIC OPINION CENTRE

We are trying to predict the results of the next election. We are not interested in how you, as an individual, vote. But we would appreciate it greatly if you filled out this form and, in private, dropped it into the interviewer's box. Thank you.

If a federal election were to be held today for the following two offices, for which candidate would you vote?

FOR MEMBER OF PARLIAMENT, FROM KINGSTON:

HENDERSON, William J.	(LIBERAL)	____
KIDD, Thomas A.	(PROGRESSIVE CONSERVATIVE)	____
MILLARD, J. Allan	(C.C.F.)	____

FOR FEDERAL SOLICITOR, FROM KINGSTON:

CARTER, Stanley F.	(PROGRESSIVE CONSERVATIVE)	____
LAVOISIER, René	(LIBERAL)	____
McINTYRE, Robert H.	(C.C.F.)	____

FIGURE 1. One form of the ballot employed in Kingston. Alternate forms omitted mention of political parties, rotated positions of fictitious candidates on the ballot, and permuted combinations of fictitious candidates and parties.

When, as sometimes occurred, a respondent remarked that he had never heard of the office of "Federal Solicitor," or did not know the candidates, the canvassers remained non-committal. When pressed on this score—a rare occurrence—they explained that they merely worked for the polling agency, and were not familiar with details of the poll. The canvassers were instructed to give the name, address, and telephone of the experimenter to any respondent who requested a fuller explanation. The one respondent who asked for this information did not in fact contact the experimenter.

Within Kingston, it was assumed, the names Carter and McIntyre would be categorized as English, and the name Lavoisier as French. Within Cornwall, the names Beaulieu and Lavoisier should be categorized as French, and Harris as English. The analyses will focus especially on votes for the odd, "contra-ethnic" candidate.

RESULTS

The Kingston sample included six hundred individuals whose ballot forms made no mention of political parties. Thus, in the fictitious contest, any deviation of the choices of these respondents from a chance distribu-

tion must be attributed either to the names of the candidates or to the order in which the names appeared. To separate these possibilities the three names were presented equally often in each of the six possible permutations.

The first point of interest is that 241 (40 per cent) of these respondents, all of whom had indicated a choice in the true parliamentary contest, failed to vote in the fictitious contest. The following analysis is thus based on the 359 respondents who did vote. Table I indicates, in terms of both number and percentage of votes, the division of votes among the three candidates, with position on the ballot counterbalanced. There are, clearly, systematic preferences among the three fictitious names. The deviation of the votes from an even three-way division was assessed by chi-square. The chi-square, with 2 df, is 82.12, p much less than .001. The Lavoisier vote, as expected, is by far the smallest; however, when McIntyre and Carter alone are considered, McIntyre is selected significantly more often ($p<.01$).

TABLE I

DISTRIBUTION OF VOTES IN KINGSTON SAMPLE WITH NO PARTIES ON BALLOT

	Total vote	Percentage
Candidate		
Carter, Stanley F.	133	37.0
Lavoisier, René	44	12.3
McIntyre, Robert H.	182	50.7
Ballot position		
First	152	42.3
Second	128	35.7
Third	79	22.0

Table I also indicates, for the same respondents, the division of votes among the three ballot positions, with names of candidates counterbalanced. There is a marked effect of position on the ballot. The chi-square, assuming the theoretical model of an even three-way split among positions, is 23.33, with 2 df, $p<.001$. The main effect is a severe depression of the third ballot position, the difference between the first two positions alone falling short of significance.

The ballots of this sample had been so marked as to permit identification of the sex of the respondent. Analysis revealed no significant differences whatever between sexes as to preferences for names or for ballot positions.

There was an additional Kingston sample of three hundred whose ballots assigned political parties to each of the fictitious candidates. There were forty-three respondents (14 per cent) who failed to vote.

Thus, in Kingston, more respondents vote in a fictitious election if candidates are assigned parties than if not ($p<.001$). The question of major interest, however, is whether systematic differences among candidates' names still occur when candidates are assigned parties. The critical candidate for analytic purposes is Lavoisier, who appeared on one hundred ballots as a Liberal, on one hundred as a Progressive Conservative, and on one hundred as a C.C.F. candidate, with ballot position held constant.

Table II presents, first, the proportion of votes for and against the Progressive Conservatives, both when Lavoisier is the Conservative candidate and when he is not. These proportions, submitted to a chi-square test, do not differ significantly. The table also indicates the proportion of votes for and against the Liberals, both when Lavoisier is the Liberal candidate and when he is not. These proportions do not differ significantly. These analyses indicate that the classifications by party and by candidate's name are independent. That is, the proportion of votes cast for a party is not significantly affected by the name of the party's candidate. This despite the fact that, in the absence of a party label, a clear order of preference exists among the names.

TABLE II

ANALYSIS OF VOTES OF KINGSTON SAMPLE WITH PARTIES ON BALLOT

	Total vote		Percentage	
P.C. Candidate	For P.C.	Against P.C.	For P.C.	Against P.C.
Lavoisier	44	36	55.0	45.0
Carter or McIntyre	87	90	49.1	50.9
Liberal Candidate	For Libs.	Against Libs.	For Libs.	Against Libs.
Lavoisier	37	51	42.0	58.0
Carter or McIntyre	82	87	48.5	51.5

The C.C.F. vote in the sample was so small (2 per cent) as to make an analysis for this party impossible. Parenthetically it may be noted that, in terms of votes for the genuine parliamentary contest, there were no significant differences among the various sub-samples to which different ballot forms were distributed.

In the French sample, in Cornwall, 126 people were given ballots which made no mention of parties. There were eighteen failures to vote in the fictitious contest (13 per cent). This percentage is smaller than that in Kingston, but no attempt was made to match the samples for socio-economic status or other variables. Table III indicates, for this sample, the distribution of votes among the candidates, with position on the ballot counterbalanced. There exists, as in Kingston, a systematic preference: the chi-square, with 2 df, is 54.06, $p<.001$. While Harris,

TABLE III

DISTRIBUTION OF VOTES IN CORNWALL SAMPLE WITH NO PARTIES ON BALLOT

	Total vote	Percentage
Candidate		
Beaulieu, Lucien	72	66.7
Harris, Robert H.	17	15.7
Lavoisier, René	19	17.6
Ballot position		
First	40	37.0
Second	44	40.9
Third	24	22.2

as expected, polls the lowest vote, Lavoisier does not do significantly better; Beaulieu is by far the preferred candidate. Table III also indicates a significant effect of position on the ballot in Cornwall, similar to that in Kingston. The third position is significantly depressed; the chi-square, with 2 df, is 6.22, $p<.05$.

There was an additional Cornwall sample of sixty-eight whose ballots assigned parties to the fictitious candidates. There were eleven failures to vote (16 per cent). This percentage does not differ significantly from the equivalent Kingston sample, or from the party-less Cornwall sample. Table IV presents, first, the proportion of votes for and against the Progressive Conservatives, both when Harris (now the critical candidate) is the Conservative candidate and when he is not. These proportions, submitted (because of the small N in some cells) to Fisher's exact test (4), do not differ significantly. The table also presents the proportion of votes for and against the Liberals when Harris is and is not the Liberal candidate; again, by Fisher's exact test, there is no significant difference between these proportions. There was only one C.C.F. vote in this sample, precluding any analysis. Thus, as with the English Kingston sample, the proportion of votes cast for a party is not affected by the name of the party's candidate, despite a marked preference among the names presented without parties.

TABLE IV

ANALYSIS OF VOTES OF CORNWALL SAMPLE WITH PARTIES ON BALLOT

	Total vote		Percentage	
P.C. Candidate	For P.C.	Against P.C.	For P.C.	Against P.C.
Harris	0	19	0.0	100.0
Beaulieu or Lavoisier	5	33	13.2	86.8
Liberal candidate	For Libs.	Against Libs.	For Libs.	Against Libs.
Harris	14	3	82.4	17.6
Beaulieu or Lavoisier	37	3	92.5	7.5

Discussion

The over-all results of the study are clear. With both English and French samples, when respondents are requested to choose among fictitious candidates for a fictitious office, they are markedly influenced both by the candidates' names and by the order in which the names appear on the ballot. Within each ethnic group, a name with ethnic connotations opposite to the respondents' is under-chosen, as is the last name in a three-person list. When, however, the same names are presented but associated with political parties, both the English and French groups are guided by party identification of the candidate, and name as such cannot be demonstrated to have a significant effect.

The precise significance of the preference orders among names is difficult to specify. Although it does not seem surprising that Lavoisier fares poorly among the English, and Harris among the French, the preferences of the English for McIntyre over Carter and of the French for Beaulieu over Lavoisier were not expected. With hindsight, it seems reasonable to assume that Lavoisier, unlike Beaulieu, is not truly a *canadien* name; indeed, a few French respondents overtly remarked that, knowing none of the candidates, they "might as well vote for a *canadien*, Beaulieu." The English respondents volunteered no bases for their choices.

The candidates' names differ, of course, in terms of sheer familiarity to the respondents, as well as with regard to ethnic connotations. Although it is difficult to separate ethnic value and familiarity, it seems worthwhile to stress that there is no reason to equate preference orders among the names with ethnic *prejudice*. The vote distribution does not indicate an active rejection of opposite ethnic groups any more than it indicates an active "prejudice" against the third ballot position. Psychologically, these data are reminiscent of experimental studies of perception; the importance of minimal, irrelevant cues is exaggerated when the normally determining cues are deliberately excluded.

The results of this study are limited by the nature of the samples and conditions employed. Whether similar results would obtain in cities of different cultural composition, for other ethnic groups, in provincial and municipal elections, and so on, is an open question. The technique employed in this study, however, seems capable of answering many such questions, and may be of value both to social scientists and to practical politicians.

We should note, finally, that the ballot actually employed in Canadian parliamentary elections is of such a form as to maximize "irrational" determinants of voting choice. The ballot does not indicate the party

affiliations of candidates, nor is position on the ballot counterbalanced. These, of course, are the conditions which maximize "ethnic voting" and the position effect. The present data, taken in conjunction with the political theory on which democracy rests, could support strong arguments for the inclusion of party affiliation on the ballot, and for counterbalancing of positions.

Summary

Within the context of a public opinion poll before a genuine parliamentary election, English- and French-speaking respondents were asked to choose among three fictitious candidates for a fictitious office. The candidates' names were varied with regard to ethnic connotations and to position on the ballot. Within both English and French groups there were marked tendencies to under-choose candidates whose names had "alien" ethnic connotations, or who appeared last on the ballot. When, however, with new samples, the same names were permuted with political parties, both English and French samples were guided by the candidates' party affiliations; name had no significant effect.

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CHANGE OF EDITORSHIP

With the publication of this issue of the *Canadian Journal of Psychology* the present editors, J. D. Ketchum and H. O. Steer, retire from the positions they have held since 1953. The new Editor, who takes over with Volume 13, is Professor J. M. Blackburn of Queen's University, and his Assistant Editor will be Professor P. H. R. James. We wish them great success and satisfaction in their tasks.

The *Journal* will continue to be published by the University of Toronto Press, but from now on all editorial and advertising correspondence should be addressed to : CANADIAN JOURNAL OF PSYCHOLOGY, QUEEN'S UNIVERSITY, KINGSTON, ONTARIO.

EFFECTS OF SELECTIVE HIPPOCAMPAL DAMAGE ON AVOIDANCE BEHAVIOUR IN THE RAT[1]

DOUGLAS KIMURA

McGill University

ALTHOUGH the hippocampus is usually thought to be a functionally homogeneous structure, some data suggest that topographical differences exist. According to Powell and Cowan (8), efferent fibers arise from topographically distinct areas of the hippocampus and project to different areas of the hypothalamus; moreover, the afferents from the lateral and medial aspects of the entorhinal cortex project to different fields in the hippocampus. Nauta (7) found in the rat that only the posterior[2] third of the hippocampus projects to the tubero-infundibular zone of the hypothalamus. Adey and his associates (1) demonstrated important projections from the dorsal hippocampus in a direction opposite to the classical fornix pathways, that is, via the entorhinal cortex to the brain stem. Finally, in the cat and monkey, electrical stimulation, through permanently implanted electrodes, of the region near the fornix seems to produce fear, while stimulation of the other end of the hippocampus has little effect (4, 6).

On the basis of these and other data it seemed worthwhile to study the effects of small, bilateral lesions in the anterior and posterior hippocampus on the acquisition and retention of an avoidance response.

METHOD

Subjects

As subjects 53 adult, male, hooded rats weighing 180 to 220 gm. were used. They were divided into 2 main groups, each containing operates and controls, as follows: Acquisition, $N = 28$ (21 operates, 7 controls); Retention, $N = 25$ (15 operates, 10 controls).

Operative and Anatomical Procedures

The anaesthetized rat was placed in the stereotaxic instrument and a scalp incision was made. Holes were drilled through the skull at the desired locations, and an electrode, consisting of a piece of nichrome wire insulated with glass except for about 0.5 mm. at the tip, was lowered into the brain to the predetermined depth. Each lesion was made by passing 2 ma. of direct current for about 15 sec.

[1]This study was financed by National Research Council of Canada Grant AP 17. Thanks are due to Dr. P. M. Milner for criticism and advice.

[2]Phylogenetic differences should be kept in mind. For example, the "anterior" hippocampus in the rat becomes the "posterior" hippocampus in the monkey.

Bilaterally symmetrical lesions were made in either the anterior or the posterior part of the hippocampus (sections 56 and 52 respectively of the Krieg (5) atlas). The scalp wound was sutured and antibiotics administered intramuscularly. Before testing, 10 days were allowed for recovery.

On completion of the testing schedule, each operate was perfused and the brain removed. After the brain had been fixed for several days, frozen sections 40 micra thick were cut, every fifth section being saved for cresyl-violet staining. The volume of destruction was estimated in arbitrary units as follows. The section showing the maximum area of damage, as measured with a planimeter, was found, and the extent of the lesion anterior to this section was measured. Then the section falling halfway between these two limits was found, and its area determined. The area of the first section was multiplied by the distance to the second section, and the area of the second section was multiplied by the distance to the anterior end of the

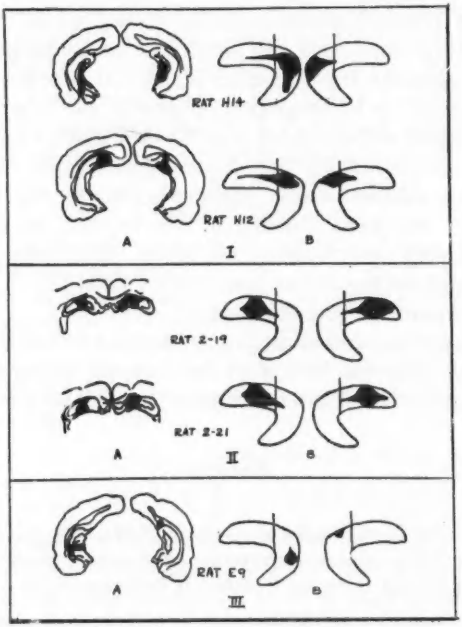

FIGURE 1. Drawings of representative sections and reconstructions of hippocampal lesions. I, posterior lesions; II, anterior lesions; III, unilateral and small lesions. A, drawings of sections showing maximal damage to hippocampus. B, schematic reconstructions of loci and extents of lesions. On the left, the left hippocampus viewed from the left; on the right, the right hippocampus viewed from the right. The lines separate the anterior from the posterior regions of the hippocampus.

lesion. This procedure was repeated for the posterior extent of the lesion, and the resulting four products were added together. Drawings of representative lesions and schematic reconstructions are presented in Figure 1.

Testing Procedures

The apparatus consisted of a straight alley 42 in. long, 4 in. wide, and 6 in. high, with wooden side and end pieces, and ¼ in. wire-mesh floor and top. A sliding door separated the start box from the rest of the runway.

All animals were maintained on a 24-hr. food deprivation schedule for 14 days, and were then trained to run to food placed in a small dish at the end of the runway. Each rat received 3 trials per day, spaced about 10 min. apart, and was allowed to eat for 45 to 60 sec. after each run. At the end of the daily testing session, the rats were fed for half an hour on a feeding stand in another room, then returned to their community cages.

"Latency" was the time required for a rat to leave the start box and touch the food. Each rat was trained until its average latency for 3 consecutive runs was 2 sec. or less. On the day after a rat reached this average, the food dish was removed, and a metal food-holder which protruded through a striated pattern was fitted to the end wall of the runway. The rat was then given one run, and after it had eaten for about 60 sec., it received a single 250 v. condenser discharge in its mouth, the metal holder and the wire-mesh floor serving as terminals. Each rat was shocked only once. The test of avoidance was a single run to the holder and pattern, either 24 hr. or 12 days following the shock. The difference between the pre-shock and the post-shock latencies was taken as the measure of avoidance; this is referred to as the "avoidance score." Thus the degree of avoidance was directly related to the avoidance score. If a rat did not eat within 60 sec., it was considered to have shown complete avoidance and was removed from the runway.

In the Acquisition Group 21 rats sustained bilateral hippocampal lesions *before* training, and, together with the control group ($N = 7$), were tested for avoidance 24 hr. following shock. On the other hand, the experimental animals ($N = 15$) of the Retention Group were operated on 2 days *after* receiving shock, and, with their control group ($N = 10$), were tested for avoidance 12 days following shock. This group also received extinction trials, which commenced the day after the avoidance test. Three such trials per day were given, spaced as during the initial training, and the number of trials required to re-establish the pre-shock latency was determined for each rat. A maximum of 60 sec. was allowed a rat on each extinction trial.

Results

Table I summarizes the test results in relation to the loci and content of lesions. Animals with unilateral or very small lesions did not differ significantly from the normal control groups on any measure.

Within the Acquisition Group, the avoidance scores of the rats with posterior lesions (range, —0.2 to 4.5 sec.) are lower than, and do not overlap with, those of the anterior-lesion (range, 48.4 sec. to complete avoidance) or the normal control rats (range, 5.6 sec. to complete avoidance). The anterior-lesion rats do not differ significantly from the normal controls.

TABLE I

LESION LOCI AND TEST SCORES OF EXPERIMENTAL AND CONTROL ANIMALS

Lesions	Acquisition		Retention		
	N	Avoidance score (sec.)	*N*	Avoidance score (sec.)	Trials to extinction
Posterior	12	−0.2 to 4.5	4	0.7 to 32.6	7.0*
Anterior	4	48.4 to complete	5	47.3 to complete	20.2
Unilateral and small	8	Complete	6	Complete	26.0
Controls	7	5.6 to complete	10	33.6 to complete	27.7

*Differs significantly from anterior-lesion and control groups, $p = .03$. Mann-Whitney "U" test.

Similarly, in the Retention Group, there is no overlap between the avoidance scores of the posterior-lesion rats (range, 0.7 to 32.6 sec.) and those of the anterior-lesion (range 47.3 sec. to complete avoidance) or the control animals (range, 33.6 sec. to complete avoidance). The posterior-lesion animals also required significantly fewer trials to extinguish than either the anterior-lesion or the normal control rats ($p = .03$). The anterior-lesion animals did not differ from the controls on any measure.

Since the Acquisition and Retention animals are comparable, they will be treated in what follows as a single group. On the run following shock only three animals, all from the posterior-lesion sub-group, showed a latency equal to or less than that recorded for the run made just before receiving shock. All other animals showed higher latencies. Thus, all except three of the posterior-lesion animals showed some avoidance; however, the degree exhibited was significantly less than that shown by the anterior-lesion and control sub-groups.

The avoidance scores and lesion sizes of the anterior- and posterior-lesion animals are presented in Table II. In order to equate these two groups for lesion size, the five rats with the largest posterior lesions may be eliminated. The lesions in these rats were quite large; a maximum lesion size of about fifty units seemed optimal for purposes of analysis since fairly extensive hippocampal damage occurred with minimal injury to other structures. The lesion sizes of the remaining posterior-lesion rats ($N = 11$; range, 12 to 51 units) are then comparable with those of the anterior-lesion group ($N = 9$; range, 18 to 45 units). Since the avoidance score of the posterior-lesion group is still significantly lower than the anterior-lesion or control groups, the deficit is attributable to the locus, rather than to the size, of the lesion.

TABLE II

LESION LOCI, RELATIVE VOLUMES, AND AVOIDANCE SCORES OF ANIMALS WITH POSTERIOR AND ANTERIOR LESIONS

Lesion locus	Rat	Lesion volume (units arbitrary)	Avoidance score (sec.)
Posterior	H8	194	−0.2
	H7	91	3.9
	E2	88	0.7
	T2	81	1.9
	E1	81	2.7
	H12	51	1.1
	E6	40	32.6
	H14	34	0.1
	3–2	31	0.0
	1–3	20	1.0
	H6	18	0.9
	E8	16	12.6
	14.3	15	0.0
	12.2	13	0.6
	18–3	13	4.5
	4–2	12	0.0
Anterior	2–21	45	(*a*)
	2–19	40	(*a*)
	H10	30	48.4
	1–15	26	(*a*)
	2–14	25	47.3
	1–3	23	(*a*)
	H5	22	(*a*)
	H9	22	(*a*)
	E11	18	(*a*)

(*a*) These animals showed complete avoidance, that is, did not eat within 60 sec. of being placed in the runway.

The lesions were concentrated within the hippocampus proper and the adjacent dentate area. It was also found that both anterior and posterior lesions occurred primarily within the dorsal aspect of the hippocampus (Figure 1). The incidental damage in the anterior-lesion group included injury to the subjacent thalamus and to the overlying cortex and corpus callosum. In some brains with posterior lesions the incidental damage affected the overlying cortex, corpus callosum, medial geniculate body, and subicular complex.

DISCUSSION

The present study shows that small, discrete lesions restricted to the posterior part of the dorsal hippocampus in the rat interfere with the acquisition and retention of an avoidance response. The effects of lesions of similar size placed in the anterior hippocampus could not be detected. These results support the notion of a topographical organization of hippocampal functions (7, 8). Although other investigators have reported deficits in avoidance behaviour as results of hippocampal injury (2, 9), the importance of the locus of damage has apparently not been noted.

In contrast to the present results, Brady and Hunt (2) found in the rat that lesions of the anterior hippocampus interfered with the acquisition of a "conditioned emotional response." This discrepancy may be due to differences in procedure, or in lesion size; unfortunately, no histological details were reported.

Brady and his associates (3) reported that hippocampal lesions had no effect on the acquisition of a conditioned avoidance response in the cat. The present experiment suggests that this negative result may have occurred because the lesions in the cats did not involve the critical "posterior" portion, that is, the area homologous to the posterior hippocampus of the rat as identified in the present paper.

Summary

Discrete, bilaterally symmetrical electrolytic lesions were made in either the posterior or the anterior part of the dorsal hippocampus of the rat. Significant interference with the acquisition and retention of a one-trial avoidance response was found to result from the posterior lesions. These data support the hypothesis that hippocampal functions are to some degree topographically organized.

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STIMULUS ALTERNATION IN PERIPHERALLY BLINDED RATS[1]

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IN A PREVIOUS PAPER Dember and Roberts (2) reported that blind rats exhibit the same amount of spontaneous alternation in a T-maze as do normal rats. This result is contrary to a prediction derived from Glanzer's (4) theory of alternation behaviour. However, it was suggested that visual stimulation is not the only potential basis for alternation. Therefore, even in the absence of visual stimulation, an animal will alternate in the T-maze, as long as the alternatives are otherwise identifiable.

An analysis by Walker *et al.* (6) suggested three potential bases for alternation: intra-maze stimuli, or S; extra-maze stimuli, or P; and the animal's prior turning response, or R. While data showed S to be the most important basis, it was hypothesized that the importance of R, for example, could be increased through appropriate manipulations. In particular, if S and P were made less salient than usual, then R would increase in importance as a basis for alternation. One possible way of decreasing the saliency of S and P would be to use blind rats.

According to the above analysis, blind rats, when tested in a T-maze, would not necessarily behave differently from normal rats, since in a T-maze S, P, and R would all lead to the same behaviour. In a +-maze, however, the relative contributions of S and P *versus* R can be separated by the following procedure. The rat is started from one stem of the +-maze on the first trial, with the opposite stem blocked; on the second trial, the rat is started from the latter stem, with the former stem blocked. If the rat alternates R, it must repeat S and P; to alternate S and P, it must repeat R. Employing this method, Montgomery (5), Glanzer (3), and Walker *et al.* (6) found R to be of little, if any, importance in accounting for the alternation behaviour of normal rats.

The present experiment tested the hypothesis that blind rats would show more R alternation in the +-maze situation than do normal rats. Further, if blinding eliminates S and P as bases for alternation, then the blind rats should show a significant amount of R alternation.

METHOD

Subjects

The Ss were 32 male albino rats, approximately 120 days old; they were obtained from the Manor Farms colony. All Ss had previously served, under the same

[1]This study was supported by a grant from the National Science Foundation. The operations were performed by Mrs. Judith Berman Levine.

conditions, in a T-maze alternation experiment. For the present experiment 16 Ss were randomly assigned to the blind group and were enucleated under Nembutal anaesthesia. The operation consisted merely in squeezing the eyeball out of its socket with a tweezers and severing the optic nerve with scissors. No further special treatment was necessary.

Apparatus

The apparatus was an enclosed plywood +-maze, 57 in. by 57 in., with a grid floor and a plexiglass top. On a given trial the +-maze was converted to a T-maze by blocking one stem at the choice point, making the functional dimensions 57 in. across the goal arms and approximately 30 in. the length of a stem. The width throughout was 4½ in., the height 5 in. The first 7 in. of each stem could be separated from the rest of the alley by means of a guillotine door; this portion served as the start box. One arm of the maze was painted black, the other white; the stems and choice point were gray.

The maze was supported 30 in. above the lab floor by means of wooden legs. It was situated in one corner of the lab, and was partially shielded from the lab's central overhead light by means of black drapes.

Procedure

The operates were allowed 2 days to recover. On the third day after the operation each S was handled for about 5 min.

On each of the next 2 days alternation testing took place. A test consisted of 2 trials in the +-maze. On the first trial S was placed in the appropriate start box, the opposite starting stem having been blocked at the choice point. After 5 sec. the start-box door was raised. S was allowed to enter one of the goal arms and was removed from the maze about 10 sec. after entry. Another 10 sec. elapsed while the block was shifted and S was transferred to the other start box. The second trial was then run exactly as the first.

On the first day of testing all Ss were started on the first trial from the south start box; on the second day all were started on the first trial from the north start box. On all trials *E* stood behind the south start box, manipulating both guillotine doors by means of fine overhead wires.

Results and Discussion

Each S was assigned a score of 0, 1, or 2, according to the number of S and P alternations it showed on the two tests. The frequency distributions of these scores, in the order above, were for the control group 1, 5, and 10, and for the blind group 2, 4, and 10. The mean score of the control group was 1.56, with a standard deviation of 0.61. The mean score of the blind group was 1.50, with a standard deviation of 0.71. The means of the two groups are not significantly different (t-test).

The mean S and P score expected by chance is 1.0. The combined mean of the two groups, 1.53, is significantly different from chance (CR = 3.48, df = 31, $p < .001$).

These results are contrary to the hypotheses that blind rats would show more R alternation than normal rats and that blind rats would show a significant amount of R alternation.

It is apparent that visual stimuli are not an essential basis of S and P alternation and that even in the absence of visual stimuli R is not an important basis of alternation. The present results corroborate the general conclusion (1, 4) that the concept of reactive inhibition cannot explain alternation behaviour.

The problem remains as to what cues the blind rat does use as the basis of its alternation.

Summary

Blind rats have been shown not to differ from normal rats in the amount of alternation they exhibit in a T-maze. The present experiment tested the hypothesis that the alternation behaviour of blind rats has as its basis the S's prior turning response. Sixteen blind rats and sixteen controls were given two massed trials per day on two successive days in a +-maze. On the first trial of each day S was started from one stem of the maze and on the second trial from the opposite stem. In this manner the relative contributions of stimulus and response elements, as bases of alternation, could be assessed.

The blind rats did not show more response alternation than the controls. Both groups showed a significant amount of stimulus alternation. The results confirm the earlier finding that visual stimulation is not a necessary basis of alternation; they add to the evidence that the concept of reactive inhibition cannot account for alternation.

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AN EXPERIMENTAL STUDY OF WHAT IS LEARNED IN A SHUTTLE-BOX SITUATION[1]

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RECENTLY, Woodworth and Schlosberg (9, p. 674) have pointed out that what is learned in "avoidance" experiments using the shuttle-box is ambiguous. For example, consider a typical shuttle-box situation, where an animal is conditioned to a buzzer to cross a hurdle from a shock-box of a given brightness to a non-shock-box of a different brightness. The two most obvious possibilities as to what is learned in this kind of situation are: A, that the animal may learn a non-discriminatory crossing-the-hurdle act; and B, that it may learn a brightness discrimination. Possibility B may be subdivided further into: B1, the animal may learn to go to a non-shock-box *merely different* in brightness from the shock-box; and B2, the animal may learn to go to a non-shock-box which is *specifically different* in brightness from the shock-box. According to the Woodworth-Schlosberg approach-avoidance criterion (9, p. 675), A and B1 would be classified as avoidance acts. (In B1 the animal presumably learns to get away from, for example, the *grey* shock-box.) On the other hand, B2 would qualify as an approach act; that is, the animal learns to approach a non-shock-box of a specific brightness. An experiment which would disentangle the above interpretations of what is learned in a shuttle-box would also have some bearing on a hypothesis put forth by Woodworth and Schlosberg (9, p. 675). They conjecture that there may be motivational shifts in situations of this kind, so that an animal may respond in the earlier phases of training on the basis of avoidance motives (for example, fear), and in the later phases on the basis of approach motives (for example, the drive for a "safe" place). Clearly, the first step in assessing this hypothesis is to find out whether there are, in fact, animals that exhibit approach behaviour at the end of training.

Following a procedure advocated and developed by Nissen (5), Campbell (1), Tolman (7), and Ritchie (6), the three possibilities outlined above might be disentangled as follows. First, condition a group of animals to a buzzer to cross a hurdle from a grey shock-box to, for

[1]This study was supported by a grant to Dr. Lambert from the General Research Fund of the University of Alberta.

[2]We wish to thank Mr. Kal Lambert for his assistance in the preliminary taming of the animals.

example, a white non-shock-box. Second, after training, replace the white non-shock-box by a black non-shock-box and test for transfer. Third, for those animals that exhibit transfer in the previous situation, replace the black non-shock-box with a grey non-shock-box, identical with the grey shock-box, and again test for transfer. Assuming that directional effects have been eliminated, those animals that have learned an approach act (B2) will presumably show no transfer under either of the above conditions. Those that have only learned to avoid a grey box (B1) will presumably exhibit transfer only in the grey-black situation. Finally, those animals that have not a learned brightness discrimination but that have learned to cross the hurdle (A) will presumably exhibit transfer in both the above test situations.

In a personal letter, Professor E. C. Tolman expressed the opinion that any of these three possibilities might be obtained under differing conditions. In the present study, therefore, three variables were manipulated: (1) brightness of the non-shock-box (white or black) during the training period; (2) shock intensity; and (3) sex difference.

Method

Subjects

The subjects were 40 naïve, virgin, male albino rats and 39 naïve, virgin, female albino rats purchased from the Greenacres Farm, a local laboratory animal supplier. Their ages ranged from 125 to 135 days.

Apparatus

The apparatus consisted of 4 boxes each 21 in. high, 17 in. long and 5 in. wide, and open at one end with the single exception of the shock-box. The latter had a 4-in. hurdle at the open end. The hurdle was wired to the same system as the shock-grid to discourage perching. The grill-floor and electrical system were built according to the specifications of Mowrer and Miller (4). The shock-box was grey and the attachable non-shock-boxes were respectively grey (the grey of the shock-box), white, and black. A guillotine screen with a bell attached and centred at the top of it was mounted at the open end of the shock-box, equidistant from the ends of the apparatus. A 200-watt electric bulb was set 2 ft. above the centre of the apparatus in a reflector slightly longer and wider than the apparatus. This was the only illumination in the windowless room used for the experiment. As far as could be determined, the light from this source fell evenly on both sides of the centre of the apparatus.

Preliminary Training

The animals were brought to the experimental room and introduced by twos into opposite ends of the apparatus, which consisted of the grey shock-box paired with either a white or a black non-shock-box. Each animal was allowed to explore freely until it had crossed the hurdle to the opposite end. No evidence of fear of the apparatus was observed in any animal.

Training Series

An animal was placed in the grey shock-box and the buzzer was turned on for 3 sec. Shock was administered automatically by means of an automatic timer 2 sec. after cessation of the buzzer, and continued (momentarily) every 5 sec. until the animal crossed the hurdle into the non-shock-box. Then the shock was turned off and the guillotine screen between the boxes was lowered to prevent retracing. The animal was allowed to remain in the non-shock-box for approximately 25 sec. after crossing the hurdle. The animals were run alternately in pairs, and each received 5 trials a day. Between trials the animal was kept in an empty pen for a 60-sec. period. The pen was located about 20 ft. from, and equidistant from, both ends of the apparatus. An exploratory study revealed that both the direction in which the animal faced when put into the shock-box, and also the hand used by the experimenter, enhanced the influence of directional cues. In the experiment, therefore, the animal was placed alternately facing towards and away from the hurdle, first with *E*'s right hand and then with his left. The animal was always placed at the end of the shock-box remote from the hurdle. Each animal received training until it made 5 non-shock hurdle-crossings consecutively.

Half of the animals (20 males, 20 females) were trained with a white non-shock-box, and the other half (20 males, 19 females), with a black non-shock-box. Each of these groups consisted of two sub-groups, one receiving a shock of approximately 1 ma. (adjusted to the size of the animal) and the other receiving a shock of approximately 2 ma. (adjusted to the size of the animal). A 1-ma. shock has been called "moderate" (cf. May (**2**)). A 2-ma. shock has been called "severe" (cf. Miller (**3**)).

The animals were run in 7 groups of 10 each and one of 9, the male animals being run before the females. For both males and females, the following order was maintained: first, the 2 groups trained with the white non-shock-box and receiving, respectively, a 1-ma. shock and a 2-ma. shock were run. Second, the 2 groups trained with the black non-shock-box and receiving, respectively, a 1-ma. shock and a 2-ma. shock were run.

Test Series

Directional test. The shock-box and the non-shock-box were reversed in position. The animal was again placed in the shock-box and the buzzer turned on for 3 sec. If, as in the previous 5 trials of the training period, the animal crossed the hurdle to the non-shock-box within the 5-sec. period, it was removed from the apparatus and kept for further testing. If it did not, it was discarded as a directional animal.

First transfer test. The shock-box, in the same position as in the directional test, was now paired with another non-shock-box to which the animal had not been exposed, and the animal was treated as in the directional test. Thus, if it had been trained with a white non-shock-box, it was tested with a black non-shock-box, and vice versa.

Second transfer test. The shock-box, in the same position as in the directional test, was now paired with the other grey box and all animals that had been successful in the first transfer test were tested for transfer within the 5-sec. period in the second transfer situation.

Results and Discussion

Sixteen of the original seventy-nine animals were discarded as directional animals.

Since, in the present experiment, each animal is tested for transfer in two situations, its behaviour can be categorized in one of three ways. An animal may show no transfer (NT); or show transfer in only one test, that is, partial transfer (PT); or, finally, show transfer in both tests, that is, complete transfer (CT). Table I shows the test results under the above headings for animals in each of the eight groups.

TABLE I

NUMBER OF ANIMALS IN EACH GROUP EXHIBITING NO TRANSFER (NT), PARTIAL TRANSFER (PT), AND COMPLETE TRANSFER (CT)

Shock intensity	Male						Female					
	Black to White			White to Black			Black to White			White to Black		
	NT	PT	CT	NT	PT	CT	NT	PT	CT	NT	PT	CT
1 ma.	3	4	2	3	2	3	2	1	2	6	0	2
2 ma.	3	2	4	5	0	2	2	2	4	4	2	3

We have to determine, first, whether any of the three variables considered in the present study had any differential effect on the transfer behaviour of the animals; that is, whether, for example, the difference in intensity of shock tended to increase or decrease the number of animals falling in a particular transfer category. Applications of the χ^2 contingency test to the data in Table I failed to show a significant difference between any of the relevant groups as regards the transfer behaviour of the animals concerned.[3] Thus none of the conditions examined in this study had any differential effect on the transfer behaviour of the animals. Hence, it is not improper to combine the various groups in order to determine which kind of transfer behaviour, if any, dominates in the present situation.

The total number of animals that showed no transfer was twenty-eight; thirteen showed partial transfer, and twenty-two showed complete transfer. In terms of the approach-avoidance criterion presented in the introduction, this means that twenty-eight animals learned to approach a non-shock-box of a specific brightness, that is, possibility B2; thirteen animals learned to avoid the *grey* shock-box, that is, possibility B1; and twenty-two animals learned to avoid the (shock) box on the side of the hurdle into which they were introduced, that is, possibility A (a hurdle-crossing act). Further analysis of the data in Table I yields the following results:

[3]We are indebted to Professor E. S. Keeping of the Mathematics Department at the University of Alberta for his advice on the statistical methods employed in the present analysis.

(1) A brightness discrimination was learned more often than a simple hurdle-crossing act. A χ^2 test, applied to 41 brightness discriminations (NT + PT) and 22 hurdle-crossings (CT), yields a figure of 5.8 which falls between the .01 and .02 levels of significance.

(2) However, avoidance acts were not learned more often than approach acts, or vice versa. A χ^2 test, applied to 35 avoidance acts (PT + CT) and 28 approach acts (NT), yields a non-significant value in either case.

(3) Among those animals that learned brightness discrimination, approach acts dominated. A χ^2 test, applied to 28 approach acts (NT) and 13 avoidance acts (PT), yields a value of 5.44 which is significant between the .01 and .02 levels.

(4) There was no tendency to learn hurdle-crossings rather than approaches to a non-shock-box of a specific brightness; nor was there a tendency to learn hurdle-crossings rather than avoidance of the grey shock-box. A χ^2 test yields a non-significant value in each case.

Result (4) is of some theoretical interest in connection with a conjecture by Professor Neal Miller. In a personal letter, Professor Miller expressed the opinion that, in our study, the hurdle-crossing act (possibility A) would be the most likely to be learned, and the approach to a non-shock-box of a specific brightness (possibility B 2) least likely. Result (4) conflicts with this prediction. In fairness to Miller, however, it should be pointed out that if the hurdle in our situation were more distinctive (it was painted the same colour as the shock-box), the tendency to learn a hurdle-crossing movement might dominate.

In regard to the Woodworth-Schlosberg hypothesis, observation of the animals that learned an approach act in the present study lends some support to the notion of a *motivational shift*. During the latter part of the training period the animals that subsequently learned brightness discriminations showed few or none of the usual fear symptoms—for example, urination, defecation, squealing, and rigid postural behaviour. The animals who learned a hurdle-crossing act, in contrast, showed exaggerated fear symptoms throughout the training period. Again, though there was no significant difference in the number of trials to criterion as between the hurdle-crossing learners and the brightness-discrimination learners, it was our distinct impression that the hurdle-crossers showed a smaller response latency (we did not measure this variable). There was a clear tendency among the brightness learners to examine sporadically both the shock-box and the non-shock-box before and after crossing the hurdle in both training and test trials. This behaviour did not occur in the hurdle-crossing learners; indeed, some of the latter ran headlong into the hurdle and then scrambled furiously

over it, in both training and test trials. This tendency to lack of fear and greater response latency on the part of the brightness learners suggests the motivational shift from fear to drive for a safe place demanded by the Woodworth-Schlosberg hypothesis.

Examination of the learning records of the animals under the various conditions reveals two interesting findings. All animals, male and female, trained with a black non-shock-box took longer to learn than did their counterparts trained with a white non-shock-box. For the former group ($N = 40$) the mean number of trials to criterion was 13.1, S.D. 4.09; for the latter ($N = 39$) the mean number of trials was 10.6, S.D. 3.56. These figures yield a t-value of 2.87 which, for 77 degrees of freedom, is significant beyond the .01 level of confidence. Subsequent examination, however, showed that the brightness difference between grey and black was less than that between grey and white. Thus, the grey-black training situation required a more difficult discrimination, which would account for the disparity.

The second point concerns the relationship between shock intensity and sex relative to rate of learning. The total number of trials to criterion under 1-ma. shock was for males, 290, for females, 202; under 2-ma. shock, for males, 219, for females, 223. These figures suggest that, in relation to the rate of learning in the present situation, shock intensity and sex are not unrelated. This conclusion is verified by the results of a χ^2 contingency test which yields a value of 8.30 which is significant beyond the .01 level of confidence.[4]

Further examination reveals that males under 2-ma. shock learn faster than do males under 1-ma. shock ($\chi^2 = 9.90$, $P < .01$) and that females under 1-ma. shock learn faster than do males under 1-ma. shock ($\chi^2 = 15.72$, $P < .01$). No other differences were significant.

Summary

Rats were conditioned to a buzzer to cross a hurdle in a shuttle-box from a shock-box of a given brightness to a non-shock-box of a different brightness. They were then tested for transfer in non-shock-boxes under different brightness conditions. Three variables, shock intensity, brightness training, and sex, were examined. None of the three variables

[4]These findings do not include a correction for the fact that one group (female, black non-shock-box training, 1-ma. shock) contained only 9 animals. The mean number of trials to criterion in this group was 11.6. However, though addition of 11.6 trials to this group results in a lower χ^2 value of 6.23, which falls between the .02 and .01 confidence levels, the general conclusion that shock intensity and sex are not unrelated is still valid. The same holds for the other statistical findings affected by this change.

examined had any differential effect on the transfer behaviour of the animals. Of the sixty-three animals in the transfer tests, twenty-eight showed no transfer, thirteen showed partial transfer, and twenty-two showed complete transfer. The results are examined in relation to the Woodworth-Schlosberg "motivational shift" hypothesis.

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DOMINANCE BEHAVIOUR AS A FUNCTION OF POST-WEANING GENTLING IN THE ALBINO RAT[1]

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IN RECENT YEARS there have been a number of investigations of the effects of early handling or gentling on weight gain (**10, 16**), emotionality (**4, 6, 16**), learning ability (**1, 6**), and resistance to a variety of stressor agents (**7, 10, 15**) in rats. The relationship between tactual stimulation as a form of post-weaning experience and later social behaviour has received very little attention in the literature. The present study investigates the effects of gentling during the post-weaning or juvenile period (**5**) on dominance-submissive behaviour in the rat. Dominance, as employed in this study, refers to that behaviour which can be elicited only when there is competition between two organisms for some goal object in which the need or drive activating the animals is congruent with the goal object, and can be satisfied by only one of the partners at a given time.

METHOD

Subjects and Apparatus

As subjects 30 male albino rats (Wistar strain), obtained at age 19 days, were divided into 2 groups of 15 Ss. The 7 litters of 4 Ss and the single litter of 2 Ss were divided equally so that the mean weight for both the experimental and the control group was 26.6 gm.[2] The Ss were housed in individual metal living cages in the Animal Laboratory, University of Toronto. Auditory and visual disturbances were maintained at a minimum. Ventilation was controlled by means of a blower system, and the temperature was held constant at 78° F. The Ss were provided with an *ad lib.* diet of water and Master Fox Chow cubes from age 19 to 43 days. All remaining food was removed from the living cages on the evening of day 43.

The Dominance Test Box was a 48 in. × 15 in. × 12 in. black wooden box with a wire-mesh lid, mounted on a table 36 in. above the floor. A metal feeding cylinder, 2¾ in. high and 1¼ in. in diameter, was fastened in the centre of the floor of the box. The opening of the cylinder was large enough to allow only one rat at a time to eat from it.

[1]This study is based upon a thesis submitted in conformity with the requirements for the degree of Master of Arts in the Department of Psychology, University of Toronto, 1957.

[2]As two Ss in each group died during the study, it was necessary to eliminate one further S from each group to maintain equality of the original litter divisions.

Procedure

The experiment was divided into 3 distinct phases:

(1) *Gentling.* The Ss in Group TS (tactual stimulation) were gentled 5 min. daily from age 21 to 41 days using the procedure described by Weininger (15). The Ss in Group NS (no stimulation) served as controls and received no gentling.

(2) *Familiarization.* The familiarization period provided a transition from the gentling period, when Ss were fed *ad lib.* in the living cages, to the dominance test period, during which they were required to eat from the metal feeding cylinder in the Dominance Test Box. It also allowed the rat to explore and become familiar with the inside of the box. This period began when Ss were 44 days old and ended when they were 57 days old.

Every morning each living cage was placed individually at the left end of the Dominance Test Box. The cage door was opened and the wire-mesh lid lowered. Each S was allowed 5 min. in which to leave the cage and to eat from the metal feeding cylinder. After this feeding period, the cylinder was sealed. The *E* then waited until S voluntarily returned to its cage. The cage was then returned to the laboratory and the feeding cylinder refilled with fresh food mash for the next rat's feeding.

Every evening the living cages were placed at the right end of the box. However, after the 5-min. feeding period, a wire-mesh carrying box was inserted into the Dominance Test Box and left there until S entered it. The rat was then removed in this carrying box to a shelf where it was allowed 30 min. to eat from a metal container of food mash. The amount of mash was far in excess of the amount that S could eat during this interval. After this feeding the rat was returned to its living cage.

Throughout the familiarization period unlimited water was available to all animals.

(3) *Dominance Test.* The dominance test period began when Ss were 58 days of age and ended when they were 69 days old. Each S in the experimental group was paired once with each S in the control group. Since it was paired once daily, each S had a total of 12 different pairings during this period. There were 12 different competitions each evening and 144 during the entire test period.

The pairings began at 7:00 P.M. and ended at 9:00 P.M., after which the rats received their only feeding of the day. Hence the rat at pairing had been deprived of food for approximately 23 hr.

Since weight has been found to be related to dominance (3, 13), Ss were weighed every evening before the first of the pairings.

The following procedure was used for the tests. The living cage bearing an experimental S was placed at one end of the Dominance Test Box and the cage of the control S at the opposite end. The placement of cages was randomized for every pairing, as was the order of pairings. Before each pairing the metal feeding cylinder was filled with fresh food mash and the floor of the box cleaned.

The cages were opened simultaneously to permit each S equal opportunity to race toward the feeding cylinder and fight for control of it. As the wire-mesh lid of the box was lowered, the 5-min. timer was set in motion. The length of time in seconds that each S spent eating from the feeding cylinder during the 5-min. period was recorded on individual stopwatches by *E*. As the opening of the cylinder was 1¼ in. in diameter, only one S at a time could eat from it. Dyes were used to facilitate recognition of the gentled and ungentled Ss.

Immediately after completion of the competition the lid of the box was raised and the cylinder sealed. Wire-mesh carrying boxes were used to remove the rats by the method outlined previously. Each carrying box contained a supply of food mash far in excess of what S could possibly eat during a one-hour feeding period. After this period S was returned to its living cage.

Results

The total time each S spent in eating from the feeding cylinder during all its pairings was divided by twelve to give a mean dominance time score for each S. These were then averaged for the two groups, TS and NS. Table I shows the group means and SD's.

TABLE I

Means and Standard Deviations of Dominance Time Scores for Gentled (TS) and Non-Gentled (NS) Groups

Group	*N*	Mean scores (sec.)	S.D.
TS	12	130	30.2
NS	12	84	31.5

As will be seen, the gentled rats dominated the feeding dish for considerably longer than the controls, and the difference between the groups means is significant ($t=2.78, p<.02$).

The mean weight of each S for the twelve-day dominance test period was calculated. There was no correlation between mean weights and dominance time scores ($r=-0.111$).

Discussion

This study shows that albino rats receiving tactual stimulation in the form of systematic gentling during the post-weaning period are more dominant than those not subject to such experience. One possible interpretation of this finding derives from the suggestion by Bovard (2) that gentling alters the balance of hypothalamic activity in favour of the anterior region, thus reducing pituitary-adrenal and sympathetic-medulla responses under stress. Apart from the possible physiological mechanisms involved, it seems reasonable to assume that the gentled Ss were less emotionally reactive to the stressful situation of fighting for food. That is, the gentled Ss with their higher emotional threshold were less fearful and disturbed than their ungentled opponents in the feeding competitions, and hence were better able to control the feeding cylinder. Previous studies (4, 6, 16) have shown that rats which have received

some form of handling prior to or just after weaning appear less emotional than unhandled controls. The present finding appears to be in line with the suggestion of Levine (9) that mice receiving no handling in infancy are less aggressive than handled mice because of greater susceptibility to emotional disturbance. With the reduced threshold to emotional stimuli, it is possible that the ungentled animal showed increased sensitivity to the presence of its competitor in the Dominance Test Box.

It should be pointed out that as soon as the cage doors were opened Ss immediately charged toward the feeding cylinder and competitions did not consist merely of alternations between pairs of competitors at the feeding cylinder; the rat in control of the cylinder would be pushed, kicked, jabbed, and butted by its opponent in the attempt to displace it. Often this activity would culminate in vigorous fighting, and the box would be filled with the leaping, rolling, bouncing bodies of the competitors. The vanquished S would attempt to escape further punishment by running about the box, or, if unsuccessful, would cower in a corner of one of the cages and submit to repeated jabs from the forepaws of the dominant S. The latter would then return to the cylinder to eat unmolested. The submissive animal, after such a severe defeat, would usually spend the remainder of the five-minute period exploring the box or circling the feeding cylinder but without approaching it closely. Sometimes a very dominant rat would temporarily surrender control of the cylinder to initiate further fighting.

The notion of a lowered threshold for emotional reactivity in ungentled rats raises a further possibility of interpretation. Mowrer (11) has postulated that fear accompanies any strong primary drive. It will be recalled that the rats competed after approximately twenty-three hours of food deprivation. In line with the work of Levine (8) on consummatory behaviour, this deprivation may have elicited greater fear in the ungentled Ss, thus causing a greater number of avoidance responses such as running, crouching, and hiding. Such behaviour would tend to interfere with the eating behaviour of the animal during competitions.

A point deserving further discussion is the age at which the gentling treatment was begun. Although some investigators (15, 16) have referred to treatment begun at weaning as "early," the author prefers the suggestion of Levine and Otis (10) that this term be reserved for the period of infancy, from birth to weaning. Levine and Otis found that rats handled before weaning (early handling) were significantly heavier, and showed significantly less mortality after deprivation, than those handled after weaning, whereas there were no differences between the late-handled (after weaning) and the non-handled groups. The present

study, however, has shown differences in dominance behaviour between rats gentled as late as the post-weaning period and ungentled controls. This raises the question whether gentling begun either earlier or later would have produced similar differences in social dominance. Levine and Otis regard the age at which handling is begun as the crucial factor, hence there might be even more marked differences in dominance behaviour between rats gentled from birth to weaning and ungentled controls. In line with the recommendation by King (5) for more complete study of this parameter (age at the time of the experience) it would also be worthwhile to determine whether there would be differences in dominance if gentling began during adulthood, say at age 100 days.

Further researches might well relate gentling and dominance to other variables such as exploratory behaviour, general activity, and ease of conditioning. Some attempts to relate dominance to other variables have already been made by Uyeno (14). He found no significant correlation between dominance scores and timidity, intelligence, or eating behaviour. However, it might be fruitful to relate dominance also to measures of prior activity or "general restlessness" (12, p. 56) as secured from a tambour-mounted cage. There may also be a relationship between handling, dominance, and ease of conditioning. Since the behaviour of the submissive animals during competitions appears to be characterized by avoidance responses, it would be interesting to determine whether there is any significant difference between dominant and submissive animals in, say, rate of learning an avoidance response in the Miller-Mowrer apparatus.

Summary

This study was designed to determine the effects of post-weaning gentling on that aspect of social behaviour known as dominance. Rats receiving tactual stimulation in the form of systematic gentling for three weeks following weaning were found to be significantly more dominant than a control group of ungentled animals. Dominance was measured by pairing each gentled S once with every ungentled S in a series of five-minute competitive feeding situations. The gentled rats were found to be significantly more dominant than the controls. This finding was interpreted in terms of the gentled S's greater resistance to the emotional disturbance aroused by the vigorous fighting for the feeding cylinder.

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EXPLORATORY BEHAVIOUR OF THE BLACK-FOOTED FERRET[1]

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IN RECENT YEARS a number of investigations have demonstrated the existence, in mammals, of behaviour not motivated by fear, thirst, or hunger, and unrelated to general activity level. These have led to the postulation of another drive—the exploratory drive. Characteristically, such behaviour is aroused by novel external stimulation; it involves either locomotor activity or manipulation of objects, and it declines as a function of time. Studies in which the relative complexity of the environment was varied have shown that rats and chimpanzees prefer more complex and novel situations to less stimulating ones (**7, 8, 21, 22, 23**). Fear (**13, 14**), hunger (**1, 11, 19**), peripheral blindness (**4**), and restriction (**12, 20**) have also been utilized as independent variables; the effects of the first three on exploratory behaviour have been studied in rats, and the effect of restriction has been investigated in both rats and dogs. Further proof that exploration operates as an independent drive is furnished by the experiments of Butler (**3**) and Montgomery and Segall (**15**) which showed that exploration could be effectively utilized as an incentive to learning.

The present study was undertaken in an attempt to obtain some measures of exploration in a previously uninvestigated species, the black-footed ferret (*mustella nigripes*). These animals, when adult, measure approximately 20 in. from head to tail and have long, slim bodies, short legs, and pointed noses. They belong to the Musteldae family, other members of which include the mink, weasel, and marten (**16**).

METHOD

Subjects

The subjects were 6 tame, experimentally naïve, adult ferrets, which were obtained from a local mink breeder who had reared them from infancy in outdoor pens. At the time of testing, the 5 females were about 1 yr. of age, and the male was 2 yr. of age. In the laboratory, they were housed in individual wire-mesh cages measuring 12 in. × 12 in. × 12 in. and were fed one meal per day consisting of ¼ lb. of canned dog food mixed with powdered milk; water was available at all times. It should be noted that 24 hr. often passed before the full ration was consumed; that is, the animals were virtually on an *ad lib.* schedule.

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Apparatus

Exploratory behaviour was studied in an open-field apparatus which consisted of an area 48 in. × 48 in. surrounded by a wall 24 in. high. The floor was ruled off into 25 9.6-in. squares. On the last 2 days, one wall was lowered so that it was 12 in. instead of 24 in. high.

Procedure

For 4 days before the start of formal testing, each ferret was removed from its cage and handled for approximately 5 min. Following this, the animals were run individually in the open-field for 5 min. each on 3 consecutive days. The following measures were taken: number of inside squares entered, number of outside squares entered; number of urinations, defecations, scratchings on floor, vocalizations, and rearings; latency (time to leave first square), time spent moving over inside squares, and time spent moving over outside squares.

On days 4 and 5 the animals were again placed in the open-field for 5 min. each and had the choice of trying to escape over the lowered wall or of investigating a cage containing a novel object placed in the centre of the field. On day 4 the novel object for half the group was a rat, and for the other half a polished brass finial. On day 5 the same objects were presented in reversed order, so that all the animals had an opportunity to investigate both objects. On days 4 and 5, in addition to the measures mentioned earlier, the following data were taken: time spent investigating the novel object, number of escapes, number of attempted escapes, and total time spent in the open-field. An escape attempt was scored if the animal got its head and forefeet over the side of the box. Total time spent in the open-field depended on whether or not the animal escaped before the 5 min. test session was over.

Results

Friedman's analysis of variance by ranks (18) showed that the over-all variance between days was significant for each of the measures in Tables I and II (the *P* values are shown in the last column of each table). Comparisons between individual days were then made and the differences were evaluated by means of the sign test. All between-days differences reported as significant were at the .03 level.

TABLE I

Number of Squares Entered, Percentage of Inside Squares, and Number of Rearings and Scratchings over 5-Day Period with and without Objects in the Open-Field

	Days					
	No objects			Objects		
Measure	1	2	3	4	5	*P**
Percentage inside squares	18.0	16.0	17.0	45.0	43.0	.01
Mean total squares	101.1	89.2	104.3	68.5	67.8	.06
Mean no. rearings	19.0	12.0	14.3	6.7	8.7	.05
Mean no. scratchings	3.7	.5	.7	.2	0.0	.05

*Friedman's two-way analysis of variance by ranks.

For both the total number of squares and the percentage of inside squares, the only significant change occurred between the third and fourth days. On the fourth day the introduction of a novel object into the centre of the open-field resulted in a significant drop in the total number of squares entered and a rise in the percentage of inside squares. For both rearings and scratchings, the only significant differences were between the first and second days; although both of these measures continued to decline, the subsequent between-days differences were not significant.

Urination and defecation (commonly used as measures of emotionality in studies employing the open-field) were almost absent, indicating either that the ferrets were not emotionally disturbed or that the measures were inadequate for these animals. Five of the ferrets did not defecate during any of the test sessions. Furthermore, it was impossible to obtain a formal measure of vocalization, since one animal vocalized persistently over fairly long periods whereas the others did so for short intervals. Other observations, however, suggest that the test situation aroused fear as well as exploration. Ferrets release a strong, unpleasant odour when frightened, and this occurred several times during the test sessions. Also, a soft cooing noise was occasionally made by some of the animals; since this sound was usually accompanied by vigorous attempts to escape from the open-field, it, too, was interpreted as a distress signal.

TABLE II

MEAN LATENCY IN SECONDS, AND MEAN TIME SPENT IN MOVING OVER INSIDE AND TOTAL SQUARES OVER 5-DAY PERIOD WITH AND WITHOUT OBJECTS IN THE OPEN-FIELD

Mean time (sec.)	Days					
	No objects			Objects		
	1	2	3	4	5	P*
Latency	10.3	7.2	4.8	2.0	2.0	.01
Inside squares	41.7	36.2	18.5	90.7	59.3	.05
Total squares	151.0	132.5	86.2	56.8	44.5	.05

*Friedman's two-way analysis of variance by ranks.

Table II shows the daily latency and the time spent moving over inside squares and total squares. Both the total time spent moving and the latency measure declined steadily from day 1 to day 5. The differences, for total time spent moving, between days 2 and 3 and between days 1 and 3 were significant, as were the differences between days 3 and 5 and between days 1 and 5. There was a gradual though non-significant reduction in the time spent on inside squares during the first three days, but the introduction of an object on the last two days in-

creased this measure, the differences between days 3 and 4 and between days 3 and 5 being significant.

An average of 75 sec. was spent on inside squares during the last two days, 65.1 sec. of which was occupied in investigation of the novel stimulus. However, significantly more time was spent investigating the rat (82.8 sec.) than the finial (47.3 sec.). The average time to leave the first square was the same in both cases, 2.5 sec. On all other measures differences between the two conditions were not significant, but, with the rat in the field, the following trends appeared: the ferrets entered more squares; urinated, reared and scratched more; spent more time moving; made fewer escapes or attempted escapes and thus spent more time in the open-field.

Discussion

The results indicate that ferrets respond to a novel situation with extensive locomotor activity; over the three-day test period this exploratory behaviour showed little decline. Furthermore, they investigated that part of the environment which contained a novel object, spending significantly more time near the animate than the inanimate object.

Some perspective can, perhaps, be gained by comparing the exploratory behaviour of ferrets and rats. Since the ferrets used in this study had been handled extensively and were quite docile, they can best be compared with tame and much-handled rats. Two recent experiments carried out at McGill with the same open-field apparatus are relevant. Experimentally naïve, hooded rats which had been gentled in infancy entered an average of 36.4 squares per session (9) while maze-bright and maze-dull hooded rats of the McGill strain (19) which had previously been run in the Hebb-Williams maze (17) were found to enter an average of 56.6 squares per session (unpublished data collected by A. E.). The ferrets, on the other hand, entered 98.2 squares per session. It is difficult to interpret this difference in view of the greater size of the ferrets, but it is apparent that ferrets are at least active as rats in an open-field. It is interesting to note that, although ferrets, like rats, prefer the outside squares, the tendency to avoid inside squares is much less marked in ferrets. The results of the two rat experiments cited above also showed that, during an average session, only 5 per cent of the squares entered by gentled rats were inside squares, while for bright and dull rats the figure was 11 per cent. The relatively high percentage of inside squares entered by the bright and dull rats is probably due to the fact that the pre-training in the Hebb-Williams maze involved crossing a large open-field to get to the food box. If the comparison is

restricted to the gentled rats, there would seem to be some basis for concluding that ferrets cross open spaces more readily than rats.

The lack of decline in exploratory behaviour during the first three days is consistent with some but not all of the rat data. Montgomery reported that rats run in a Y-maze for approximately ten minutes each on five consecutive days showed a within-days but not a between-days decline (**10**). Williams and Kuchta (**23**), similarly, stated that exploration in a Y-maze increased over a period of six days. On the other hand, Glickman (**4**) found both a within-days and a between-days decline in exploration of the same apparatus by both blind and sighted rats over a three-day interval. In the two studies referred to earlier in which the open-field was used, bright and dull rats and gentled rats continued to explore at a steady rate for three days. However, the exploratory activity of the control animals run by Mogenson and Ehrlich (**9**) declined steadily, and the difference from the first to the second day was significant. Although it is difficult to account for the discrepancy between Glickman's findings on the one hand and those of Montgomery and Williams and Kuchta on the other, the data on open-field exploration by gentled and control animals and by bright and dull rats suggest that the amount of handling is an important variable in the day-to-day changes in exploration. Corresponding findings for dogs are reported by Thompson and Heron (**20**) who found that, while exploration decreased for both groups, dogs which had been reared in a free environment showed less between-days decline than dogs which had been reared in restriction. The results of the present experiment lend further support to the hypothesis that increased handling leads to a slower decline in exploration, although no conclusive statement can be made since no one, to our knowledge, has tested wild ferrets under similar conditions.

Finally, the responsiveness of these animals to novel stimuli is consistent with the accounts of perceptual curiosity in the rat (**2**), "manipulatory drive" in monkeys (**5, 6**), and play in the chimpanzee (**21, 22**).

Summary

Six tame, experimentally naïve, black-footed ferrets were run for five days in the open-field. On the last two days, a novel object was placed in the centre of the open-field, and the subjects were given an opportunity to explore or to escape. Ferrets were found to respond to a strange situation with extensive locomotor activity which showed little decline over the first three days. During the last two days, significantly more time was spent in that part of the open-field which contained a novel object. The resemblance between the exploratory behaviour of ferrets

and rats is noted, and it is suggested that increased handling may lead to a slower between-days decline in exploration.

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PERFORMANCE IN A VIGILANCE TASK AS A FUNCTION OF INTER-STIMULUS INTERVAL AND INTERPOLATED REST[1]

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THE FIRST systematic experimental studies on vigilance were reported by Mackworth (5). He and others have used the term "vigilance" to describe tasks which require the subject to respond, over extended periods of time, to a sporadically presented stimulus.

Of the various experimental findings, the most common has been that performance deteriorates progressively throughout an experimental session (1, 2, 5). In addition, a significant rise in the level of performance has been demonstrated to follow periods of interpolated rest (1, 5).

In each of the above three studies, the over-all order of presentation of stimuli was the same for all subjects, with a particular stimulus sequence being repeated throughout successive portions of the task for each subject. In addition, performance was scored in terms of missed stimuli, the stimuli being of near-threshold intensity. Thus, as performance became progressively worse, the intervals between stimuli underwent an apparent lengthening. Had these experiments been designed to prevent the possible confounding of the effects of order and length of inter-stimulus interval with those of task duration and rest, the findings might have been different.

Assuming that the results of the above studies are not artifacts of experimental design, they may be explained by postulating an inhibitory factor, somewhat analogous to Hull's (4) reactive inhibition construct, which develops during an experimental session and dissipates during periods of interpolated rest. If this is true, length of inter-stimulus interval should have no differential effect on performance. On the other hand, if inhibition is response-generated, dissipating between stimulus presentations, performance should be an increasing function of length of inter-stimulus interval. Such a finding would also be predicted from Deese's (3) hypothesis that "expectancy" increases as the mean of all previous inter-stimulus intervals is approached, continuing to increase as the interval grows beyond the mean. Whereas performance is generally assumed to be inversely related to the amount of accumulated inhibition, Deese regards it as directly related to the magnitude of expectancy.

[1]Defence Research Medical Laboratories Report No. 234–1, DRML Project No. 234, P.C.C. No. D77–94–20–42, (H.R. No. 157).

The purpose of the present study was to investigate performance in a vigilance task as a function of task duration, length of inter-stimulus interval, and degree of interpolated rest. Order effects were controlled by presenting the stimuli in as many random orders as there were subjects. To avoid confounding the effects on performance of the progressive change in the apparent length of inter-stimulus interval with those of task duration, all stimuli were presented sufficiently above threshold to be consistently detected, the dependent variable being response time.

Method

Subjects were 60 Canadian Army recruits. Each S was instructed to depress a microswitch when a light was seen through an aperture 1 cm. in diameter placed at a distance of 15 ft. from S. Response time to the light was recorded by a Hunter Klockounter, and the duration of the light (100 msec.) was controlled by a silent Hunter Decade Interval Timer. Every precaution was taken to eliminate cues which might allow S to anticipate the onset of the light.

A 3-min. period of instruction preceded the experimental session. The S was instructed to keep his thumb on the switch and to depress it as fast as possible each time a light appeared. He was asked to do his best throughout the session, which he was told would last for approximately one hour. After these instructions, the light was presented 7 times at 10-sec. intervals, S's response time being recorded on each occasion. The purpose of this short practice period was to determine whether or not S understood the instructions, and also to obtain a covariate for possible use in future experimentation.

After the practice period, the light was presented 51 times to each of the 60 Ss, the intervals between stimuli being 30, 45, 60, 75, and 90 sec. The over-all inter-stimulus interval order was different for every S, with the restriction that all Ss experience each of the 5 intervals once every 5 min. The interval sequence in any 5-min block was selected at random from the 120 possible sequences.

Of the Ss 20 spent 50 min. without interruption on the task. Another 20 Ss rested 5 min. before completing the final 10 min. of the 50-min. task; and a third group of 20 Ss rested 10 min. before the final 10 min. of the task. Subjects of the latter two groups were provided with popular magazines and allowed to smoke during the rest intervals.

Results and Discussion

Figure 1 shows response time as a function of task duration and degree of interpolated rest. It is apparent from the figure that performance deteriorated progressively throughout the first forty minutes of the task and improved after rest, the greater improvement following the ten-minute rest period.

The results of an analysis of variance of response time for all Ss on the first forty minutes of the task are presented in Table I. The performance decrement over time was statistically dependable at the .001

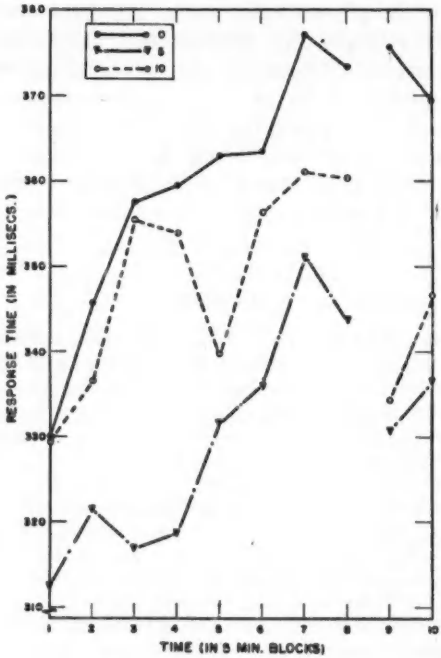

FIGURE 1. Response time as a function of task duration and degree of interpolated rest. (The points on each of the curves represent group means, each group consisting of 20 Ss. The legend refers to length, in minutes, of the rest periods interpolated between blocks 8 and 9.)

level. The interval and interval × time interaction effects were both non-significant.

To determine the reliability of the observed effects on performance of the five- and ten-minute rest periods, a *t* test of the difference between the mean response times, based on the last five minutes before and the first five minutes after rest, was made for each group. The *t* ratios were 1.77 and 3.23 for the five- and ten-minute groups respectively. The improvement following five minutes' rest was signficant at the .05 level, while that following ten minutes' rest was significant at the .005 level. Both tests were based on related measures and employed only one tail of the *t* distribution. The function relating amount of improvement in performance to the three degrees of interpolated rest, when subjected to

TABLE I

ANALYSIS OF VARIANCE OF RESPONSE TIME FOR ALL SUBJECTS ON THE FIRST 40 MINUTES OF THE TASK

Source	df	Sum of squares	Mean square	*F*
Subjects (S)	59	7,069,618.3		
Time blocks (T)	7	337,247.7	48,178.24	14.56*
Inter-stimulus interval (I)	4	21,293.3	5,323.32	1.61
TI	28	98,965.3	3,534.48	1.07
ST	413	1,773,251.3	4,293.59	
SI	236	744,747.7	3,155.71	
STI	1,652	5,097,718.7	3,085.79	
TOTAL	2,399	15,142,842.3		

*Significant at the .001 level.

trend analysis, was found to be significant at the .025 level. Because of the differences among the three groups in over-all level of performance on the first forty minutes of the task, the effects of which may have interacted with those of rest, the exact form of the function relating amount of improvement to rest cannot be specified.

The post-rest data for each of the three groups were also subjected to analysis of variance. The effects of time and length of inter-stimulus interval, as well as the interaction effects of these two variables, were found to lack statistical significance.

The findings of the present study support the hypothesis that inhibition accumulates continuously throughout the duration of a vigilance task, dissipating during periods of interpolated rest. These findings are incompatible with both the expectancy hypothesis and the hypothesis that inhibition is response-generated.

SUMMARY

Sixty Canadian Army recruits served as Ss in a vigilance task lasting 50 min. Each S was instructed to fixate on an aperture 1 cm. in diameter placed 15 ft. from him, and to depress a switch as fast as possible whenever a light was seen through the aperture. The light was presented 51 times to each S, the intervals between stimuli being 30, 45, 60, 75, and 90 sec. Of the Ss 20 spent 50 min. on the task without interruption; 20 Ss rested 5 min. before completing the final 10 min. of the task; and 20 Ss rested 10 min. before the final 10 min.

Response time increased significantly throughout the duration of the session, but was invariant with length of inter-stimulus interval. Performance improved following rest, the amount of improvement being some monotonic function of degree of interpolated rest. The findings

support the hypothesis that inhibition accumulates continuously throughout the duration of a vigilance task, dissipating during periods of interpolated rest. These findings are incompatible with both the expectancy hypothesis and the hypothesis that inhibition is response-generated.

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TWO ORIENTATIONS IN PSYCHOLOGICAL RESEARCH: EFFECT-CENTRED AND CONDITION-CENTRED

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THIS ARTICLE defines two distinct orientations to experimental psychological research. It shows how these orientations differ in their requirements for the design and conduct of research, and argues that applied research is at present weakened by the failure to distinguish between them.

Definition of the Two Orientations

Effect-centred orientation. Here the research problem is to produce or maximize a given desired effect by any means possible. It is assumed henceforth that there are available adequate "criterion" measures of the degree to which the desired result is achieved, for example, of proficiency in job performance, change of attitude in a given direction, or alleviation of a personality maladjustment. But, whatever the desired result, producing it is paramount, and justifies anything done to produce it, within socially permissible limits. Strictly speaking, it does not matter whether the manipulations successful in producing the effect are deduced from theory or stem from guesswork. Nor does it matter whether they have some reasonable and clearly understood relation to the effect produced (for instance, better lighting conditions may improve learning) or seem completely irrelevant and unrelated (for instance, standing on one's head might conceivably improve certain types of learning).

The "effect-centred" orientation is obviously synonymous with "applied research" as this term is commonly understood. Taken strictly and seriously, unalloyed with methodologies appropriate to the goals of other types of research, it can provide one intelligible and defensible orientation to the conduct of psychology.

Condition-centred orientation. The research problem here is to assess or compare the effects of individual variables, whatever these effects may be. For example, the problem may be to compare and contrast the effects of two conditions, X and Y, on learning or on attitude change. The hypothesis tested may be that X will produce greater effects than Y. However, this is still not an effect-centred orientation, because the production of *maximum* effect might require applying several conditions at

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once, lumped together in such a way that it is impossible to assess the separate effects of each condition. Moreover, the comparison of X and Y may depend on the different *types* of effects they produce, rather than primarily on their relative magnitude of effect along any single response dimension.

In the *effect-centred orientation, conditions or independent variables are subservient to effects*—important only as they contribute to maximizing a desired effect. In *condition-centred research, effects are subservient to conditions*—important only in so far as they provide a means for evaluating and contrasting these conditions.

Differences in the Design and Conduct of Research Required by the Two Orientations

Let us now consider, in simplified form, some alternatives for the design and conduct of manipulative psychological research. Let A, B, C, D, X, and Y represent each of six different independent variables or conditions which may be applied to produce e_1, e_2, etc., as effects or responses. There are then two types of design for any one experimental run. (*a*) The one is "mass-application," where two or more conditions are applied jointly in such a manner that the effects of any one of them on the e_1 response cannot be determined. This may be schematized as $A + B \rightarrow e_1$. (*b*) The second is "separate-application," where one or more conditions are applied in such a manner that the effects of each can be separately assessed, but the combined effects of more than one of them cannot be assessed. This may be schematized as $A // B \rightarrow e_1$.

With an *effect-centred orientation*, the researcher can use either a mass-application or a separate-application design. He will use the $A + B$ mass-application design when he has confidence beforehand that: (*a*) conditions A, B, etc., each produce some effect in the desired direction, and (*b*) these conditions when jointly applied do not interfere with each other so seriously that $A + B$ will have less effect than A alone or B alone.[2] In other words, the applied researcher should vary and apply at one time all conditions which are known to meet assumption (*a*) in order to maximize effects in the desired direction. He should not employ a separate-application design, although he may be tempted to do so—perhaps, for example, to determine exactly *how much* effect each of the component conditions have, or to make the research seem more "scientific" by testing a hypothesis (such as that A produces more effect than B).

The applied researcher should use an $A // B \rightarrow e_1$ or separate-applica-

[2]In the writer's opinion, this latter assumption will be warranted in such a vast majority of cases that it need be included here only for purposes of complete logical presentation, and is discussed no further.

tion design to the extent that he is ignorant of the separate effects of A or B in the desired direction. Clearly, mass-application of A, B, etc. is unlikely to maximize effect when component conditions may act negligibly or in opposite directions on the effect. Separate-application research will determine which conditions act in the correct direction and which do not. The former should then be used in a mass-application design.

How does one decide when foreknowledge of effects of A, B, etc. is so uncertain that a separate-application design is needed? No rigorous rules exist; but, in the writer's opinion, there are frequently good reasons for moving quickly or even initially into mass-application designs without overlong pause to solidify foreknowledge in separate-application sub-experiments. First, there are practical difficulties which restrict the number of sub-experiments an applied researcher can conduct. There is the labour and expense of procuring subjects, and of constructing and administering experimental treatments. Moreover, there is often powerful administrative resistance to experimental interference with ongoing operations in the area with which research is concerned. In such a situation, where only one or two experimental runs may be permitted, the applied researcher should give preference to mass-application of conditions whose effects are likely to be in the correct direction over separate application of conditions whose effects are unknown and unpredictable. He should also be more willing to risk including in his mass-application design sets of conditions whose effects are less certainly known. There is a wide range of types of conditions where this risk is not really severe. For example, in attempting to maximize learning in a classroom, there are certain types of conditions which can almost always be counted on to improve or at least not decrease learning—conditions such as improved lighting or obvious improvements in organization of the material presented.

In summary, applied research should centre on mass-application designs, always as a culmination, frequently even in the initial stages of experiment. Separate-application designs will sometimes be needed to establish or extend knowledge of the set of component conditions known to act in the desired direction (later to be combined in mass-application). But the applied researcher must beware of tarrying too long at this stage simply because he wants to be "scientific" and test a hypothesis comparing A and B, when he should be combining them for maximum effect.[3]

[3]We have emphasized manipulative ("experimental") research in our discussion, but psychometric correlational research also provides examples of effect-centred mass-application *vs.* separate-application designs. For example, research has shown a factor of anxiety, loaded both by a "willingness to admit common faults" test and by a "tendency to agree" (that is, give "yes" responses) measurement (1). The rela-

It may be suggested that a factorial design will compress into one grand design the separate-application and mass-application phases discussed above, since the cells will contain all possible combinations of the presence or absence of component conditions. Conceivably, such a design will sometimes be useful, but this will not usually be the case, for several reasons. First, there are typically at least a few conditions about which the applied researcher has foreknowledge. To maximize effect, these conditions must be represented in joint or mass-application cells, which means that to complete the factorial design they would also have to be represented in separate-application cells.[1] But this latter would be wasted effort from an applied research point of view, since foreknowledge is already available. On the other hand, if we are in equal and substantial ignorance of the effects of all component conditions, A, B, C, D, etc., the complete factorial design will still involve wasted effort, since it will be likely to include some mass-application combinations of A, B, C, and D which were not optimal in maximizing effect—for instance, A + B + C + D, when C and D turn out in separate-application to be of negligible or opposite-to-desired effect.

The wasted effort described above can become truly formidable. For, as the applied researcher casts his net wide to find as many effect-producing conditions as possible, the number of cells in the factorial design increases enormously with corresponding increase in the time, labour, and expense of procuring subjects and setting up treatments for each of the separate cell sub-experiments. The increased difficulty of interpreting tests of significance for higher-order designs is a further reason for avoiding the full factorial design in such cases. In summary, it seems more likely that, if separate-application designs are required at all, they will be one-by-one runs. The results of these runs, not the need to fill out the factorial design, will decide which combinations of these conditions are later to be used in mass-applications.

With a *condition-centred orientation* to effects, the researcher will use a separate-application design: $X//Y \rightarrow e_1, e_2$. Essentially, such research starts by a deduction or inference from some theoretical proposition, resulting in a prediction that X will produce different effects than Y. (Y

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tions of each variable to anxiety were discovered in the first place by a separate-application design: that is, one in which "tendency to agree" and "willingness to admit" measurements were arrived at independently of one another. Analysis of such separate relationships has theoretical significance for understanding the nature of anxiety, but can also contribute to effect-centred research, where the desired effect is maximization of the relation to anxiety, keeping test duration as short as possible. This effect can be achieved by designing a mass-application test where faults are always admitted via a "yes" response. In such a test the "willingness to admit" and "tendency to agree" variables will combine to maximize the anxiety loading, but they cannot be individually assessed and compared.

may be nothing more than absence of X.) The results of this comparison either confirm the theoretical proposition or force modifications in it, and the cycle of hypothesis and empirical verification is ready to begin again. A mass-application (X + Y) design will not be used, since it does not permit separate evaluation and comparison of X and Y.[4] Although the required design is a separate-application one, such as occurs in the early stages of effect-centred research, there are differences both in the choice of conditions to be applied (X//Y instead of A//B) and in the choice of effects (e_1, e_2 instead of e_1). That is, the conditions chosen to be varied in theory-confirmation experiment will often differ from those chosen to be varied in applied research, and so will the dependent variables or effects. Often the former will be more "molecular" and less lifelike. Thus, theory-testing research might use as stimuli the presence, degree, and timing relationships of light flashes and air puffs, and, as effect, the frequency of eye-blinks. The applied researcher, who usually deals with problems of relatively immediate and direct significance to humans, is unlikely to be interested in the frequency of eye-blinks as a response to be maximized, or in light flashes and air puffs as stimuli which may produce practically signficant effects in any response-characteristic of interest to him. Moreover, condition-centred research may sometimes evaluate conditions by their effects along several distinct response dimensions, while applied research typically concentrates on one criterion at a time.

Summary and Conclusions

Two distinct orientations to psychological research are defined: (*a*) *effect-centred*, where attention focuses on the dependent variable, that is, on producing and maximizing a given effect, by whatever means are discoverable; and (*b*) *condition-centred*, where attention focuses on the independent variable, that is, on the evaluation and comparison of various conditions via the differences in effects produced, whatever these effects may be.

These two orientations require differences in method. (1) Effect-centred research should proceed as soon as possible to mass-application of several or many conditions at once; condition-centred research will never, or almost never, do this. (2) Condition-centred research always

[4]There are, of course, "hypotheses" to the effect that X + Y + Z will maximize a given effect. But, in the writer's opinion, these usually amount only to applied research guesses, with interest centred in maximizing the effect, not in evaluating differences in conditions via the effect. If there exist genuinely theoretical, condition-centred propositions of the type, "X + Y will maximize effect," the writer does not know of them and believes they must be very rare.

employs a separate-application (X//Y) design. Effect-centred research may use this design in its preliminary stages, but even then typically differs or ought to differ from condition-centred research: (*a*) in its choice of independent and dependent variables, and (*b*) in moving as soon as possible to a mass-application design.

In the writer's opinion, it is most unlikely that the theoretically oriented researcher will weaken his research by choosing the mass-application format or variables of an applied research type, but the applied researcher *does* make the converse error. He cannot or will not recognize that effect-centred problems require designs and variables that are not in the classical, laboratory-restricted, theory-oriented tradition of experimental psychology. This tradition is entrenched and has high status; it is "controlled," "scientific," "real research"; anything else is "mere engineering" (and less polite names have been suggested). Therefore, in the effort to make his research scientifically palatable by just a dash or two of "analysis" plus a garnishment of theory-related variables, the applied researcher often fails to take advantage of a situation which would permit successful, effect-producing mass-application. He may be found testing the hypothesis that A produces more or different effects than B, when he actually should be applying both together for maximum effect. Seduced by theories which have no direct or obvious relation to the desired effect, he may manipulate independent variables which test such theoretical propositions but have little or no effect in the desired direction. Properly, however, his only "theorizing" should concern the nature of conditions likely to produce a given desired effect, and this alone should govern his choice of variables.

In the writer's opinion, there is at present very little middle ground for effect-centred research whose variables, data, and design have general theoretical application, or for theoretically chosen conditions and response variables which are also of immediate practical significance for the applied researcher. Such a convergence is to be hoped for, and poses a special problem for researchers and research methodologists. But the applied researcher must solve pressing effect-centred problems in the here and now. The methodological and practical importance of these problems are such as to warrant *now* their own special outlook and procedures.

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ABILITIES AND INTERESTS: II. VALIDATION OF FACTORS

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A PREVIOUS PAPER (5) described a factorial analysis of a battery of psychological measures of ability and of interest. Centroid analysis of the matrix of intercorrelations of twenty-four variables, and rotation of axes by the method of two-dimensional sections, produced a slightly oblique structure of eight factors. One of these was defined as an ability factor, three as interest factors, and the remaining four as composite factors. It was suggested that this result supported the contention that interests and abilities as measured by psychological tests and inventories are aspects of more complex overlearned behaviour patterns, properly called traits.

This paper describes a method used to validate the interpretation made of the factors, and the results. The identification of the factors as traits is based on the definitions that Eysenck (4) and Allport (1) give to that term, and is intended in particular to emphasize their dynamic characteristics, and the fact that they are more generalized than habits.

The subjects of this study were all college freshmen. It was possible, therefore, to attempt to validate the factor interpretations by reference to both preference and achievement data. The preference data consisted of the initial faculty of registration of every student. Since faculty registration occurred before the testing programme it was entirely independent of the results thereof. The achievement data consisted of the final grade point average at the end of the first year of studies of each student. After those students who withdrew from college during the session were eliminated forty-seven subjects were left in Arts, thirty-five in Commerce, and thirty-six in Science.

METHOD

Each student in the above three groups was assigned a "derived factor score" for each of the eight factors. These scores were obtained by summing the products of: (*a*) the loadings of each variable with a significant loading, and (*b*) the *T*-score of each individual for each factor. Calculation of multiple regression coefficients and use of the resulting beta weights might have provided more exact estimates of factor scores. However, since all the variables (except sex, which was not used in calculating any derived factor scores) were *T*-scored, the problem of differing variances did not arise. Since a factor loading may be thought of as a quantification

of the contribution of any given variable to the composition of a factor, the product of factor loading and *T*-score can serve as a measure of the contribution of any given score to the total factor score of an individual. This method fails to allow for the intercorrelations of the variables involved, but the error so introduced does not do undue violence to the resultant analysis. The method amounts to a variant of Cattell's (3) "situational index" method.

The mean derived factor score, the standard deviation, and the product-moment correlation between factor score and grade point average were calculated for each group (Arts, Commerce, Science) on each factor. These may be found in Table I.

TABLE I

DERIVED FACTOR SCORE MEANS, STANDARD DEVIATIONS, AND CORRELATIONS WITH GRADE POINT AVERAGE, BY FACULTY

		Group[1]				
Factor		Arts ($N = 47$)	Commerce ($N = 35$)	Science ($N = 36$)	Male ($N = 100$)	Female ($N = 35$)
A	Mean	43.4	18.8	38.2		
	S.D.	24.5	14.3	18.5		
	Rxy	.08	−.05	.12		
B	Mean	23.2	22.3	21.4		
	S.D.	2.3	3.6	4.8		
	Rxy	.00	.09	.39*		
C	Mean	0.3	1.1	18.0		
	S.D.	15.6	13.8	13.1		
	Rxy	.16	.28	.18		
D	Mean	80.0	89.7	81.6		
	S.D.	14.0	11.4	11.4		
	Rxy	−.02	.45**	−.05		
E	Mean	18.2	8.5	9.2		
	S.D.	14.1	13.0	10.1		
	Rxy	.41**	.21	−.01		
F	Mean	−21.6	−17.2	−15.7		
	S.D.	7.3	6.4	6.0		
	Rxy	−.02	.30	−.07		
G	Mean	40.8	38.9	35.9	37.4	41.7
	S.D.	7.9	4.9	7.0	6.4	7.6
	Rxy	−.29*	−.41*	−.05		
H	Mean	47.1	51.3	50.5	51.3	44.8
	S.D.	8.4	9.2	7.2	7.8	7.9
	Rxy	−.54**	.30	.24		

[1]The sum of the Arts, Commerce, and Science sub-groups does not equal the sum of the Male and Female groups since individuals who withdrew during their first year of study were eliminated from the former groups.

*Significant at the 5 per cent level.

**Significant at the 1 per cent level.

Results

Factor A. This factor was equated with Cattell's factor QI.Ib: "Guiding people for their own presumed good *vs.* being coldly objective" (2). It was exclusively a preference factor, contrasting likings for activities involving looking after, training, teaching, etc., with those emphasizing interviewing, convincing, supervising, etc. This suggests that those with high Factor A scores would register in Arts, those with low scores in Commerce.

The differences in mean scores between the Arts and the Commerce students, and between the Commerce and the Science students, are significant at the $P = .001$ level. The Arts–Science difference does not approach significance, nor do any of the correlations between factor scores and grade point averages.

Factor B. This is the general ability factor. Since none of the preference variables shows significant loading, there seems little basis for anticipating differences in Factor B scores between the groups, and in fact none of the differences is significant.

It is interesting to note that the only substantial correlation between factor score and grade point average is that for the Science group, which is significant at the $P = .05$ level.

Factor C. This factor was defined as "thoughtful interest in understanding nature *vs.* being the center of attention." The positive pole carried preferences for discovering, investigating, planning, being a chemist, psychologist, etc. The negative pole was illustrated by preferences for selling, collecting, entertaining, being a sales manager, radio commentator, music teacher, etc. As would be expected, the Science group showed a high average score on this factor, Arts and Commerce low. The differences in mean scores shown in Table I between Arts and Science and between Commerce and Science are both significant at the $P = .001$ level. The Arts–Commerce difference is not significant, nor are any of the correlations.

Factor D. High scores in this business interest or accounting factor clearly have favourable implications for Commerce registration. Such scores indicate both superior ability in, and preference for, computational activities. It may be anticipated also that there should be some correlation between scores on this factor and achievement within a Commerce programme.

The statistics in Table I show that the Commerce group averages significantly higher scores on Factor D than either of the other two groups. The correlations for Arts and Science grade point averages are not significant, but that for the Commerce groups is significant at the $P = .01$ level.

Factor E. The positive pole of this factor, which was established as a verbal activities preference, would be expected to be associated with registration in Arts. The negative pole, defined as "manipulation of materials" would seem more significant for registration in Commerce or Science.

The differences in mean scores for this factor are significant at the $P = .001$ level for both the Arts–Commerce and the Arts–Science differences. The grade point average coefficient for the Arts group is also significant at the $P = .01$ level.

Factor E is a preference factor, since none of the ability variables achieved significant loadings on it. This makes the correlation for the Arts group even more suggestive, since in this case measures of interest are functioning as predictors of achievement.

Factor F. This factor was defined as a composite of socialized religion and verbal fluency *vs.* irreligious aesthetic interests and spatial visual ability. The negative pole–socialized religion and verbal fluency–clearly implied an Arts registration. The opposing preference-ability constellation did not have such clear implications. None of the programmes of study available to this group seems particularly appropriate to it.

The differences in group means for the Arts–Commerce and Arts–Science comparisons on Factor F are significant at the $P = .01$ and $P = .001$ levels, respectively. The three correlations and Commerce–Science differences are not significant.

Factor G. The positive pole of this factor was identified as a composite of confident self-assertion, abstract reasoning ability, and female sex. High scores on the factor would be expected in Arts and Commerce students, low scores in Science students. These expectations are based partially on the implications of the factor definition, and in part on the fact that Arts study programmes attract a higher proportion of female registrants than do either Commerce or Science programmes. (Of the thirty-five females in the sample, twenty-seven registered in Arts.) It may also be anticipated that females would average higher scores on Factor G than males. (It should be noted that sex was not included among the variables used in the calculation of the derived factor scores for either Factor G or Factor H.)

The mean Factor G scores shown in Table I are significantly different for Arts–Science at the $P = .01$ level; for Commerce–Science at the $P = .05$ level; and for sex difference at the $P = .01$ level. Of the three correlations those for the Arts and Commerce groups are both significant at the $P = .05$ level.

Factor H. As with Factor G, "confident self-assertion" is associated with the positive pole of this factor, in this case as part of a constellation

including superior numerical and spatial-visual abilities. The negative pole was identified by superior memory ability and female sex.

In the absence of any preference variable which would be suggestive for faculty registration, the prediction that both Science and Commerce students would show higher average scores is based on the implications of the ability variables. The mean Factor H score for males can also be expected to be higher than that for females.

The sex difference in mean scores for Factor H is significant at the $P = .001$ level. The Arts–Commerce difference is significant at the $P = .05$ level, and the Arts–Science difference falls just short of this level. It is interesting to note that the coefficient of correlation for derived factor score *vs.* grade point average for Arts students is negative, and significant at the $P = .01$ level, whereas those for Commerce and Science are positive, although neither of the latter is significant.

Discussion

The first paper in this series (5) adduced evidence for the hypothesis that there were substantial relationships between measured interests and measured abilities. It further suggested that if such relationships were demonstrated they might properly be considered to be traits of personality.

Evidence has been put forward in this second paper which shows that in terms of "derived factor scores" significant relationships exist between the factors and both academic achievement and choice of study programme. Although data have been examined for all eight factors, those for Factors D, F, G, and H (on which both ability and interest measures converge) are of most relevance for the trait interpretation of the factors.

Allport, in his discussion of traits (1), specifies a set of criteria which such attributes of behaviour should meet in order to properly be called traits. The first of these specifies that a trait must have more than nominal existence. This requires that traits be defined operationally. The second requirement is that traits be more generalized than habits, that they be systems of integrated and organized habits. Third, traits are required to be dynamic, capable of directing responses. The fourth and fifth criteria specify that traits be capable of identification through statistical or empirical methods, and that they need show only relative independence. The critical use of conventional moral and ethical concepts as guides in the search for traits, and the avoidance of unrealistic demands for consistency in human behaviour are also included in the list. Finally, Allport specifies that traits may be studied clinically, for the

role that they play in a single personality, or through techniques of population-sampling and individual difference.

The composite factors of this study seem to satisfy all of Allport's criteria rather elegantly and may properly be considered as personality traits with integrative and dynamic characteristics. The theoretical considerations of Ferguson and Murphy cited in the previous paper (5) converge on overlearned behaviour patterns as the most acceptable definition of what ability and interest tests measure. The present study supports the hypothesis that abilities and interests are but aspects of overlearned behaviour patterns properly called personality traits.

Summary

A centroid factor analysis of a battery of ability and interest measures produced eight factors, which, when rotated to simple structure supported the hypothesis that interests and abilities were related. Calculation of factor scores on each factor for each member of the sample allowed the relationships between the factors and both academic achievement and choice of study programme to be determined. These relationships, and especially those involving the factors on which interest and ability measures converged, met requirements for statistical significance. The evidence was interpreted as supporting both the contention that measured abilities are aspects of more basic dimensions of personality, that is traits, and the contention that these traits have integrative and dynamic characteristics.

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BOOK REVIEWS

The Hangover: A Critical Study in the Psychodynamics of Alcoholism. By BENJAMIN KARPMAN. Springfield, Ill.: Charles C. Thomas [Toronto: Ryerson Press], 1957. Pp. xxiv, 513. $10.50.

THE AUTHOR adopts the position that addiction to alcohol always has its roots in pre-alcoholic neurotic involvements. He contends that in the hangover state the neurotic components of the personality are greatly magnified. Consequently, "Hangovers provide the *via regia* into the emotional life of the alcoholic and . . . by studying the problem of hangovers one may gain some insight into, and a more intimate understanding of the psychodynamics of alcoholism." As the drinker's alcoholic career progresses, hangovers become increasingly painful. This ". . . is not due to the headache, the nausea, the cold sweats, the chills and fever, or even the 'shakes' but to the emotional pain which accompanies them—the guilt, anxiety, self-accusation, the sense of hopelessness and despair. . . ." In an effort to escape from these accentuated manifestations of his neurosis the drinker has recourse to more alcohol, only to encounter them again in even more exaggerated form. The result is that once the drinking has begun a cycle is set up in which alcohol is taken to alleviate the intolerable emotional pain consequent upon previous drinking. This "cyclical flight" is terminated when the drinker becomes too ill to continue or when he has been completely purged ". . . even if only for a brief period, of all the unhealthy emotions that originally were part and parcel of the conditions of alcoholism." The severity of the hangover does not depend solely upon the amount of alcohol ingested or upon the type of beverage drunk, but varies with the emotional state of the drinker.

The author's views are supported by an intensive clinical study of seven male and seven female alcoholics. These case studies are unusually thorough and well worth reading. However, it should not be imagined that Karpman's subjects constitute a representative sample of alcoholics. For example, no fewer than five of the seven male subjects exhibited manifest homosexuality. Although reliable estimates of the prevalence of such disorders among alcoholics are lacking, Dr. Karpman's sample is almost certainly atypical in this regard at least. Consequently, the author is not justified in extending his special findings to generalizations concerning all alcoholics. The fact that he has done so can only contribute to the existing confusion in the field.

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Readings in Perception. Edited by DAVID C. BEARDSLEE and MICHAEL WERTHEIMER. Toronto: D. Van Nostrand Company (Canada) Ltd., 1958. Pp. xiii, 751. $9.50.

THIS WELL-CHOSEN COLLECTION is most welcome for the literature on perception is more scattered than that on any other central topic in psychology. The fifty-two selections are drawn from twenty-five different journals and books, some not easily accessible, some in French or German. The temporal span is from 1910 to 1957, but more than half the items date from the 1950's—evidence of the recent revival of interest in the field.

Most of the papers report controlled experiments, but theoretical discussion is well represented, from the early Gestalt observations to Gibson's reformulation of depth perception, Helson's adaptation level, Werner and Wapner's sensory-tonic theory, and recent probability interpretations. The whole series, though drawn from such different sources, leaves a remarkable impression of integration—of wide agreement on the problems concerned, ingenious and varied attacks on them in half a dozen countries, and the emergence, if not yet of a comprehensive theory, at least of many established principles. No better example of psychology at work could be given to the serious student.

This rich collection includes important classical contributions such as Rubin's 1915 figure-ground article, Max Wertheimer on principles of organization. David Katz on surface, film, and volume colour, and Michotte on perceived causality. More recent materials are represented by articles on the testing of numerous Gestalt and other hypotheses, accounts of the Ames phenomena, and (from this journal) of the McGill experiments on isolation and early restriction, Hochberg's summary of the Cornell symposium, and key papers on the "perceptual defence" controversy and on the effects of reinforcement, need, and expectation. The volume ends appropriately with Bruner's masterly 1957 paper, "On perceptual readiness."

Scarcely any of the papers have been abridged—a great improvement over certain books of "readings"; instead, space has been saved by omitting the treatment of sensory equipment (except for Morgan's excellent summary) and delving only slightly into the personality field. All told, this is a stimulating presentation of first class psychological work and thought, and should draw some young researchers away from the overcrowded terrain of conventional learning studies.

J. D. KETCHUM

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Psychoanalysis: Evolution and Development. By CLARA THOMPSON. New York: Grove Press [Toronto: McClelland & Stewart], 1957. Pp. xii, 252. $1.95.

THE APPETITE of the beginning psychology student for psychoanalytic fare is notorious. The harassed instructor reacts in several ways: he chokes the beginner on references to the originals, gives him a personally prepared light lunch, or ignores him and hopes he won't starve before the emergence of a diet falling somewhere between Freud and *Life*. Such a diet has been provided by Dr. Thompson. In the increasingly significant Evergreen Series of paper-backs, she makes available to students a considered review of the origins and status of psychoanalytic theory. Her bias is within the cultural interpersonal stress of the William Alanson White Institute—Horney, Fromm and Sullivan in particular, with some of Adolf Meyer's training in evidence. In the first two chapters the author presents as good a brief history of psychoanalysis and statement of Freud's fundamentally biological position as one could wish for junior students. Several chapters on anxiety, resistance, transference, the ego, and character structure are handled traditionally, but with a constant emphasis on cultural context and interpersonal theory. These lead logically to the author's personal position (not identified as such), and an evaluation of Freud's cultural orientation in the light of more modern ideas concerning instinct theory. The remaining chapters deal with the "deviants"—Jung and Adler, Rank, Ferenczi, and Reich—together with a remarkably lucid introduction to Horney, Fromm, and Sullivan. A concluding chapter on therapy is redundant.

The chief value of this volume is as a reference for students, not as a systematic contribution to analytic literature. Classical psychoanalysis is presented fairly, if not enthusiastically, and is given perspective by contrasting it at each step with alternate views. References for these views are frequently elderly (for example, Malinowski, 1927; Ferenczi 1916), but respectably so, and sadly missing in most students' vocabulary. The introduction to interpersonal theory as represented by Sullivan provides an excellent background for considering Freudian hypotheses. The major weaknesses lie in the neglect of studies of learning, and in the sometimes naïve acceptance of anthropological opinions as scientific data. However, the text emerges as a happy medium between history and a presentation of problems. The thoughtful student will gain the emphasis that most teachers desire: there are hypotheses to be tested, not just doctrines to be memorized.

H. O. STEER

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On Human Communication. By COLIN CHERRY. New York: John Wiley & Sons Inc., 1957. Pp. xiv, 333. $6.75.

THIS BOOK is a *tour de force* by the Henry Mark Pease Reader in Telecommunications at the University of London. The author modestly describes it as a simple introduction for newcomers to information theory, but it is doubtful whether anyone else (except the prodigious Norbert Wiener himself) would have the breadth of scholarship, technical knowledge, and versatility of mind needed to produce it. Cherry is as well read in Descartes and Locke as he is in Hebb, Shannon, and Carnap; he deals as readily with the mathematical theory of communication as with the nature of meaning and perception. The chapters, originally given as lectures, are delightfully readable, full of unexpected allusions and illustrations, and written with beautiful clarity. Psychologists may find some sections difficult, a few too simple; but no one curious as to what information theory is all about could hope for a better introduction.

J. D. KETCHUM

University of Toronto

Explorations in Awareness. By J. SAMUEL BOIS. New York: Harper and Brothers, 1957. Pp. x, 212. $2.75.

IN the 31 chapters crowded into about 200 pages are many words of wisdom and much charm. Dr. Bois has strained Alfred Korzybski's general semantics through a sieve of his own rich experience, humanity, and humour, and what he presents makes rewarding reading. This is not merely an exposition of Korzybski or general semantics, but rather a reflection of the life and work of Samuel Bois. It is by no means a simple "How to live" book, but the serious reader who works his way through the author's exposition will find many points of practical value. However, these have to be assimilated to be used, for there are no simple rules.

Who should read this book? Scientists by all means, especially psychologists who may have lost their way in a jargon which hinders the clarity of their thinking. Students in all areas, business men, indeed anyone who is interested in human behaviour and thought; all these will find this short book both challenging and informative.

KARL S. BERNHARDT

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